SCIENTIFIC AMERICAN
No. SUPPLEMENT 119

Scientific American Supplement, Vol. V, No. 119
Scientific American, established 1845

NEW YORK, APRIL 13, 1878.

Scientific American Supplement, $5 a year.
Scientific American and Supplement, $7 a year.

THE FLOW OF METALS.

BY DAVID TOWNSEND.

In investigating the subject of the punching of metals, and the consequent flow which occurs, no exact law could be determined covering all cases, owing to the limited number of experiments made, and also to the want of technical knowledge on the subject. M. Tresca, in *La Poinçonnage des Métaux*, published some experiments on the punching of iron, but his results cannot be accepted as complete and accurate, owing to the fact that the means at his disposal were limited; those that were arrived at were for the action of a large diameter of punch upon small thickness of metal.

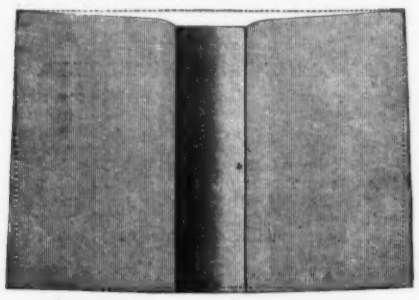


FIG. 1.

His experiments were made with a punch of 1·18 inch diameter (30 mm.), on iron plates, the greatest thickness being ·669 inch (18 mm.), and the least ·1968 inch (5 mm.). He announced, as the result of his investigations, the general law that "*when pressure is exerted upon the surface of any material, it is transmitted in the interior of the mass from particle to particle, and tends to produce a flow of metal in the direction in which the resistance is least.*"

The following experiments were made with nuts, or bars, punched, cold, by Messrs. Hoopes & Townsend, and are intended to show that an actual flow does take place in metals under pressure, which flow is governed by some law not yet enunciated.

The first experiments were made with rectangular blocks of iron, which were planed accurately to the dimensions

FIG. 2.

given in the table below, and then punched, cold, through the centre of the block.

	BEFORE PUNCHING.			*AFTER PUNCHING.*				
	WIDTH.	THICK.	LEN'H.	WIDTH.	THICK.	LENGTH.		
				TOP.	BOTTOM.		TOP.	BOTTOM.
No.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
1	1·745	1·748	2·51	1·775	1·82	1·748	2·511	2·562
2	1·742	1·748	2·51	1·768	1·814	1·748	2·510	2·552

From the above table it is seen that, as the punch entered, a flow took place, which was greatest in the width—the direction of least resistance—the length being but slightly increased. By comparing the widths at the top and bottom, we see that the increase has been greatest on the bottom face of the block. From this we should conclude that the flow was greatest at the bottom of the piece; but such is not the case, for the block, resting against the immovable bed of the machine, could not curve down as could the metal at the top, and was, therefore, compelled to spread laterally, producing this increased width.

FIG. 3.

If we examine the punched block, we notice that it has bulged in the width, producing a curved surface, concave toward the axis, greatest at the central line of its width, and decreasing gradually in either direction as it departs from that line.

The length is also increased, but not as noticeably as the width. When the punch first touched the block, the whole upper face was depressed (Fig. 1), the surface converging toward the hole as the lowest point, in the immediate vicinity of which the metal curved sharply downward, like water flowing into an orifice. This depression can be seen in Fig. 1, which is a longitudinal section, the dotted lines being the dimensions of the block before punching, and the black lines after. We also see that the block has become wedge-shaped, and is slightly convex on the bottom face.

If now we look at Fig. 2, which represents the core punched from the block, we see that it is only $1\frac{1}{16}$ inch in depth, while the hole from which it came was $1\frac{3}{4}$ inch thick. At first sight, it would seem that all the metal from the hole had been squeezed into the core, and, therefore, that its density must be increased directly as this ratio of thicknesses. To prove that such was not the case, a piece of metal was chipped from block No. 1, and its density taken, with the following result:

Weight in air	3·78	gms.
" " water	3·297	"
Loss	·483	"

$\therefore \frac{3{\cdot}77}{{\cdot}483} = 7{\cdot}82$ = density of the block.

The core itself was then treated in a similar manner, giving:

Weight in air	14·9965	gms.
" " water	13·07	"
Loss	1·9265	"

$\therefore$ 1·9265 = 7·78 = density of the core

FIG. 5.

From this it appears that the density of the block is slightly more than that of the core; but this difference is probably due to the density of the surface being increased by chipping and filing, and also to the preponderance of matter in the block over the core. As the density has not increased, there must have been a flow of metal from the core into the block, the amount of which we will now determine.

Three experiments were made with blocks of the same thickness and diameter of hole, and the volumes of the resulting cores were determined by displacement, on account of their being irregular bodies. All the experiments being made with the water at a constant temperature, no correction is necessary for differences of temperature.

Core from block No. 1:

Weight in air	14·9965	gms.
" " water	13·07	"
Loss	1·9265	"

FIG. 6.

The weight of the water displaced is therefore 1·9265 gms., and is equal in volume to that of the body producing the displacement. As 1 gramme of water exactly equals 1 cubic centimeter, the displaced water has a volume of 1·9265 cubic centimeter, which is also the volume of the core. Now as 1 cubic centimeter = ·06103 cubic inch, the volume of the core in inches is equal to ·11757295 cubic inch. If, now, we take the measurements given in the table, and calculate the volume for a hole of $\frac{7}{16}$ inch in diameter, we find:

V = ·3773294 cubic inch = volume of hole.
V = ·1175743 " " = volume of core.

·2597551 " " = volume of the metal which has flowed into the block from the core in the process of punching, and corresponds to 68·844 per cent.

Core from block No. 2:

Weight in air	14·864	gms.
" " water	12·953	"
Loss	1·911	"

This loss is equal to a volume of ·1166283 cubic inch. The volume of the hole being same as before, we have:

V = ·3773294 cubic inch.
V = ·1166283 " "

·2607011 " " = volume of metal flowing, which is equal to 69·09 per cent.

The third example was a finished hexagon nut, having the same thickness as before and the same diameter of hole.

Core of No. 3:

Weight in air	15·154	gms.
" " water	13·220	"
Loss	1·934	"

FIG. 7.

The volume of the core is therefore equal to ·117974 cubic inch, and we get:

V = ·3773294 cubic inch
V = ·1179740 " "

·2593554 " " = volume of metal flowing, or 68·73 per cent.

Similar experiments were tried with other thicknesses; but the flow was not so marked, and seemed to decrease directly as the diameter of the hole increased and as the thickness of the bar decreased.

FIG. 4.

In order to show to the eye the flow which had thus occurred, several of the large nuts with their cores were piled and rolled together, to make up the bar. It will be noticed that they all curve downward, and that the greatest curvature occurs at the top, remaining nearly constant for some distance, and then decreasing toward the bottom. This shows that the flow must have occurred when the punch first entered the bar, and was continued regularly, until the pressure above parted the under face, and the core was forced out. If we examine at the core, we find that at the top, where the punch first strikes the block, it is concaved upward, and that the laminæ composing it are forced in curved layers before the punch, until the resistance of the lowest part of the metal was overcome, when we find that the curves get more and more flat, until at the bottom face there is a slight concavity, due to the flow which took place through the opening in the die.

FIG. 8.

In the case represented in Figs. 5 and 6 the hole was punched *with* the grain, instead of *across* it, the result being that the superposed laminæ, instead of being curved downward, were wrinkled or warped, from the flow and the consequent pressure which took place, acting against their sides or faces.

Acting on suggestions obtained from the above results, several experiments were tried by partially punching bars of the same thickness with punches that had the same diameter, but which varied in length according to the depth of the hole to be punched. The bars were uniformly 1¼ inch thick, and the punch ⅞ inch in diameter; the following were the results obtained:

1. The first bar was punched to a depth of exactly ⅞ of an inch; the resulting piece was then planed in half, polished, and etched with acid, when its surface had the appearance of Fig. 7. Here we see that, although the punch has gone half way through the bar, it has hardly marked the under surface. If we observe the lower portion of Fig. 7, we see that some of the layers have not yet been bent out of shape, and, therefore, that the marking of the under surface could not have been due to the flow or to the starting of the core,

FIG. 9.

but was caused by the general pressure of the punch on the body of the metal over the hole in the die.

2. The next bar was punched to a depth of 1⅛ inch, and on being treated with acid, as before, it showed the form of Fig. 8. We find that the mark on the under face now projects ⅛ of an inch, and that the lowest layers are slightly disturbed; therefore, the point of maximum flow has been reached, and must lie between ⅞ inch and 1⅛ inch in depth, or, in other words, the starting point of the core, for the above thickness of iron, is about 1 inch from the upper surface of the bar.

3. In the third experiment, under exactly the same conditions, the punch was entered into the bar to a depth of 1¼ inch. Referring to Fig. 9, we see that here the core has started, and is projecting ⅛ of an inch below the under face

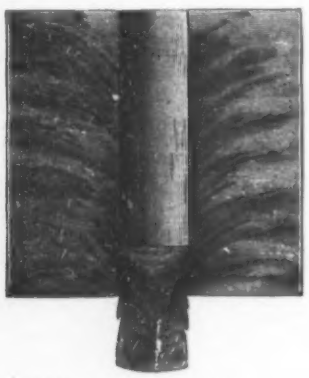


FIG. 10.

of the block. The layers are all bent, the lower ones being the least, and the upper ones, where the punch has passed, showing the flow, as in the case of complete punching represented in Fig. 3. We also see that the core has already attained its least thickness, being but ¾ of an inch in depth, which is about that of the core represented in Fig. 4.

4. In the fourth and last experiment the punch was stopped at a depth of 1½ inch, the resulting block being shown in Fig. 10. The core projects from the bottom face nearly ¼ inch, and measures, as before, almost ¾ inch in depth. The layers, in this case, are all severed, and the line of parting of the core from the block is plainly visible. The force necessary to finish the punching is very small, compared to the total force required to punch such thickness of metal. The point of the maximum flow of metal is visible to the eye when watching the action of the machine, which gives a slight jar, starting downward, as if relieved from great pressure, when the core just starts, or wholly divides the under surface of the block.

That these experiments prove the flow of metal to occur in cold punching, and at great depths, is undeniable, but whether it will in the future be practically applied to take the place of partial drilling, is a matter which time only can determine.

This property of flowing in downward curves is highly advantageous to the quality of the nut, as, on being tapped, the thread is made up of several layers, instead of one, and the strain which comes on them is taken on end instead of across the grain, thus giving the iron a much greater resisting power. The process of punching these thick bars does not depend for its successful performance upon the time taken, but upon the accuracy and power of the machine and the quality of the punch. The element of time is introduced only so far as the wear and tear of the tools and the machinery determine it; as for the flow, that remains the same, whether the motion is fast or slow.—*Journal Franklin Institute.*

MANUFACTURE OF WELDED IRON TUBES.

IN the ordinary method of manufacturing welded iron tubes or pipes a plain strip of iron, or skelp or strip bent more or less into a tubular form, is heated in a furnace to a welding heat, and by the action of the rolls or other pressing machinery the edges of the heated skelp are welded together. In this method of manufacture the whole of the iron of which the tube or pipe is to be made is raised to a welding heat, and grave inconveniences arise therefrom, especially in the manufacture of tubes made from thin strips, or having little substance of iron in them. By the invention of Mr. Edward Roden, of Compton, near Wolverhampton, these inconveniences are avoided. Instead of heating the whole of the strip or skelp of iron, he heats only the edges which are to be welded together, and the parts adjacent thereto. In order to effect this he constructs a furnace having a long narrow opening at its top, and the strip of iron from which the tube is to be made having been bent into a tubular form, with its edges overlapping, is placed upon the said opening in the furnace, the said overlapping parts being alone exposed to the strongest heat of the furnace, while the greater part of the tube being external to the furnace is but slightly heated. Near the end of the furnace, a bulb mandril, supported on a rod, is situated, the axis of the said bulb mandril being in a line with the axis of the partially formed tube on the furnace.

The end of the mandril rod is fixed to a wagon capable of traveling on a draw bench; underneath the bulb of the mandril is a pressing roller, which is pressed upward or against the bulb of the mandril by a spring or weight, or otherwise. A pair of tongs connected to the wagon of the draw bench or other drawing mechanism take hold of the partially formed tube at its upper side, and motion being communicated to the said chain draw bench or other drawing mechanism, the partially formed tube is drawn off from the furnace on to and over the bulb mandril, its overlapping and heated edges being pressed between the bulb of the mandril and the roller underneath, and thereby welded together. After the welding of the skelp has been effected the mandril and its rod, together with the tongs and welded tube, are removed to a sufficient distance from the furnace to allow the tube to be removed from the mandril; the mandril and other parts are then restored to their original positions near the furnace, ready to act upon the weld a second skelp. By means of this invention welded iron tubes or pipes can be readily and quickly made, and from thinner iron than can be conveniently used in the ordinary method of manufacture.—*Mining Journal.*

THE NELSON.—The Nelson, armor-plated double-screw ship, was taken in the Channel lately to try her engines at half boiler-power for four hours, under the superintendence of Captain Carne, of the Reserve, Mr. J. Oliver, of the Admiralty, and Mr. J. Cory, chief inspector of machinery, and officers of the steam reserve. The engines were in charge of Mr. Dunlop, of the firm of Elder & Co. The results were as follows: First half hour, revolutions 63·8, indicated horse power 3,157; second half hour, revolutions 62, horse power 2,929; third half hour, revolutions 63·9, horse power 3,052; fourth half hour, revolutions 64, horse power 3,112; fifth half hour, revolutions 63·8, horse power 3,072; sixth half hour, revolutions 63·8, horse power 3,117; seventh half hour, revolutions 63·7, horse power 3,117; eighth half hour, revolutions 63, horse power 3,080. The results were: Mean horse power, 3,080; consumption of coal, 1·47 lb. per indicated horse power; speed of ship, 12·43 knots. The engines worked in a satisfactory manner; there were no signs of hot bearings of any kind, nor were the boilers primed. Of the ten boilers only six were used. In a similar trial of the Northumberland the horse power was 2,990; consumption of coal, 1·99 lb. per horse power; speed of ship, 12·10 knots; the Nelson thus, with less coal, developed more horse power and attained greater speed. The Nelson has two cylinders, of which the high-pressure is 60 ins. in diameter and the low-pressure 104 ins. in diameter; length of stroke, 3 ft. 6 ins., with surface condensers.

SUCCESSFUL RAISING OF A SUNKEN VESSEL.—The Salvage Steamship Company, London, Captain Coppin managing director, succeeded recently in raising and floating into dock at Portsmouth the Alpheba, a fine iron ship of 1,300 tons, wrecked in November on the Demsridge Ledge, off the Isle of Wight. The vessel, which was only on her second voyage, was insured for £13,000, and was bought for £200. She was raised by the construction inside her of wooden platforms, a marvel of engineering skill. Another company had previously abandoned the task. Captain Coppin is contractor for raising the Vanguard, which he will attempt in June, and is very sanguine of success.

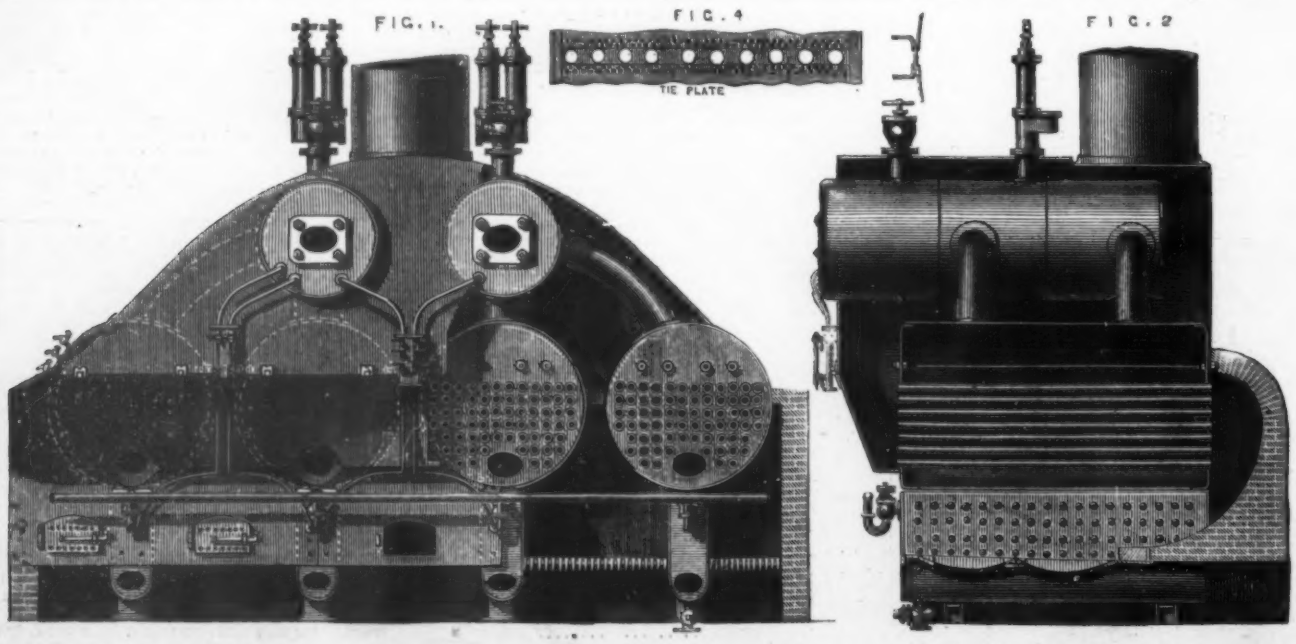
HIGH-PRESSURE MARINE BOILER.

WE give engravings of a form of high-pressure marine boiler designed and patented by Captain G. J. Scott, of Calcutta. The chief feature consists in the fixing, to the bottom of a cylindrical tubular boiler, of a water leg or chamber, which extends down below the level of the fire bars. In the particular case we illustrate there is a group of four boilers, all fitted with these water legs, the fire grates being arranged between the water legs and between the outer water legs and side walls. These small outer grates can, however, be dispensed with.

The water legs are properly stayed and are connected to the main shells of their respective boilers in the manner shown by the detail views. The holes shown in the tie plate at the point of junction are quite sufficient to secure proper circulation. The feed water is admitted to the water legs and directed down below the level of the fire bars, the water legs thus forming depositing chambers in which a large proportion of the solid matters in the water is thrown down, and from this deposit can be removed by blowing off and cleaning out through the hand holes provided for that purpose.

Above the boilers are placed two steam drums suitably connected to the four main shells, drain pipes being led from the bottom of these steam drums down to the water legs, so as to convey back any water carried up with the steam. The space between the adjacent main shells is bridged over by a short firebrick arch, and the products of combustion pass first along the sides and bottom of the boilers, then back through the tubes, and finally over the tops of the main shells and around the steam drums to the chimney. The water legs prevent the accumulation of deposit over the fire, and the boilers being short there is no trouble experienced from unequal expansion, while the position of the fire-hole door prevents cold air from striking on the bottom of the boiler when the door is opened.

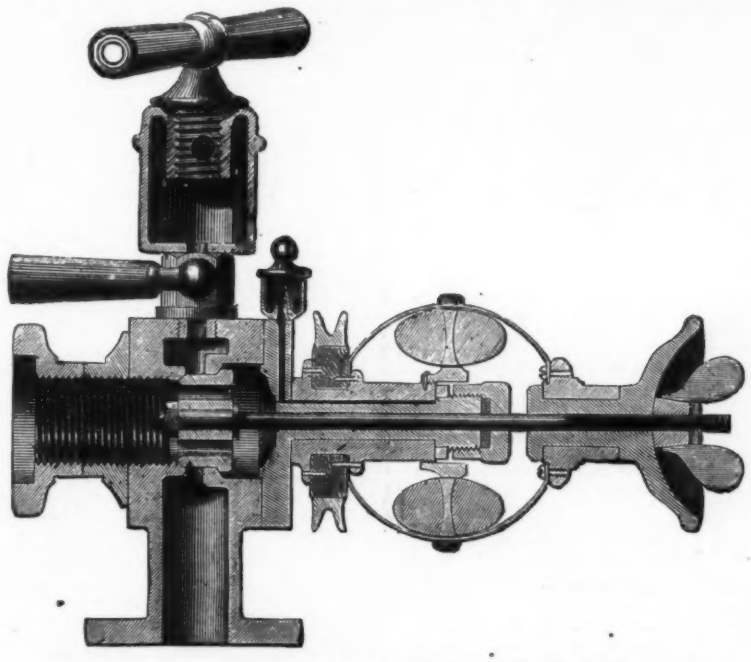
The vessels to which these boilers have already been fitted have not surface condensers, the boilers being supplied direct from the Ganges, the water of which river is—in some seasons especially—very dirty. The boilers have thus been tried under unfavorable circumstances, but the reports we have heard of their performance are very satisfactory. One set of the boilers above mentioned, we may state, is working in the steamship Progress at a pressure of 125 lbs. per square inch. Captain Scott has also a pair of these boilers, with one grate between them, working in a steam launch at 100 lbs. pressure. Captain Scott informs us that the weight of these boilers is about half that of ordinary marine boilers of the same heating surface, an important point in light draught vessels, while the boilers are also much more handy for transportation than the ordinary type.—*Engineering.*



IMPROVED HIGH-PRESSURE MARINE BOILER OF LIGHT WEIGHT.

IMPROVED GOVERNOR.

WE give below a cross section of the governor which John Bourne & Co., of London, have, during some years past, been applying to their high-speed engines. The whole structure is made of brass polished. The spindle, which is horizontal, carries two bent springs, at the center of which two brass weights are situated, and the spindle, springs, and weights are rotated by a small pulley driven by a cord from the shaft. The spindle carries a double-beat valve which is closed or opened by the centrifugal action of the weights, or the centripetal action of the springs. The latter is a constant quantity, but is overcome more or less by the centrifugal force as the speed of the engine varies, thus regulating with great accuracy the admission of the steam. The double-beat valve acts also as a stop-valve, its position being governed by the hand wheel at the end of the spindle, while the periphery screw prevents this wheel from moving from the position at which it is set. On the top of the governor there is a grease-cup, by which oil or grease is continuously fed in with the steam. The thumb screw at the end serves to regulate the speed of the engine, or to stop and start the latter.—*Engineer.*



IMPROVED HIGH SPEED ENGINE GOVERNOR.

EGYPTIAN OBELISKS AND THEIR RELATION TO CHRONOLOGY AND ART.*

By BASIL HENRY COOPER, B. A.

EGYPT is the monumental land, and among her most characteristic mouments are her obelisks. From the earliest times they were reared in pairs at the entrances of tombs and temples, the monotony of whose long horizontal lines these handsome upright shafts broke, in deference to a true instinct of architectural art. Had they not been thought things of beauty they would not have been carried off to adorn Nineveh, Rome, Constantinople, Paris, and London. Nearly all the obelisks we know or read of were cut from that ridge of fine granite, very free from flaws and shakes, through which the Nile plows its way, nearly under the tropic of Cancer, to form the first cataracts. The ancients called it Syenite, after the place, now Assouan, near which were quarried during thousands of years these monolith needles of flame-colored stone, which Pliny tells us were meant to represent the rays of the sun, and were dedicated to that divinity. They were the oldest idols, and he was the oldest Egyptian god thus worshiped. They have been defined as rectangular prisms tapering upward and culminating in a pyramidion, which rose about one-twelfth the whole height, and each was placed on a pedestal, also said to have tapered, which was about another twelfth. As early as under the Fourth Dynasty they are seen on the tops of truncated pyramids, and occasionally even surmounted with the sun's disk. This renders the notion of the imitative solar ray more intelligible. We are to think of a broadening beam of light shot downward by that luminary. We are thus helped to look at the Great Pyramid itself—comparing it with the little pyramid which crowned every obelisk—as representing the flood of sunshine in which the mummy of Cheops lay basking in hope of a final resuscitation. The religious idea best explains the fact that the obelisks were always monoliths, each a single stone, not built up bit by bit, like our modern imitations. Neither cost nor toil was to be spared on an image to be consecrated to the god. Hence, too, the care with which they were polished, and the consummate art with which the Egyptians provided against a drawback thence arising. They had observed that the play of light upon a polished surface made it seem concave, although perfectly level, and they accordingly gave the faces of their obelisks a degree of convexity exactly proportioned to this optical illusion. The French mathematicians have estimated this convexity in the instance of the Paris obelisk at 16 lines at the center of the arc. Modern engineers, with hydraulic lifts and other mighty machines at their command, are still the first to marvel how these Titans of old managed to coax such enormous monoliths out of the quarry, to lug them such prodigious distances, not always by river or canal, from their stony bed, and then to rear them on their pedestals. The indications afforded by the unfinished obelisk, between thirty and forty yards long, still *in situ* at Assouan, are thought to shed much light upon the problem of extraction. The form of the obelisk, it seems, was first roughly outlined by cutting longitudinal gashes in the granite rock. Three of the sides were then pared into shape and polished, while the fourth was left still clinging to its native bed. To detach the shaft in its entirety, deep grooves were cut lengthwise underneath, into which were squeezed wooden wedges. These wedges, being moistened, swelled, thus loosening the stone. On drying, they were squeezed further in, and again wetted; and this process, being often repeated, split the granite at last, without any shock, in the direction wanted. This method seems preferable to that adopted by the Hindoo quarrymen to separate the Seringapatam obelisk, as described by Sir John Herschel In that instance, the groove was intensely heated, and then suddenly filled with water. The Egyptians next dragged the obelisk out of the quarry, laid it upon a sledge, and, plenty of water being poured upon the sandy path along which it had to travel, in order to harden the ground, the monolith glided along, drawn by large relays of men and animals. For water carriage it was brought to a dock by the riverside, laid on a raft between two heavily laden barges, which being afterward lightened, it floated down the Nile at the next inundation. How the Romans transported obelisks by sea—to be sure, not across the Bay of Biscay—we have scarcely any details; indeed, none to speak of. Pliny (H. N. xxxvi. 9) says ships were built for the purpose. He adds that Augustus ordered that which brought the first—therefore, either the Flaminian or that in the Piazza di Monte Citorio—to be laid up for ever afterward, as a marvel, in the dockyards at Puteoli; so it was evidently deemed an immense feat. In Pliny's time, Augustus' obelisk ship had been already burnt. On the pedestal of the Atmeidan obelisk at Constantinople there are some bass-reliefs which seem to refer to its transshipment. In a paper lately read before the British Archæological Association, Dr. Birch was in some doubt whether what is represented there is an ordinary Roman galley, or something so like Mr. John Dixon's cylinder ship as to give that gentleman good reason to look to his laurels.

The question of the erection of the obelisks in ancient and modern times is one far too large and too difficult to allow of more than a bare mention here. Most likely it was not until after they had been reared that the engravers began their toilsome work, scaffoldings being used for the purpose. They patiently inscribed the hieroglyphical texts, as drawn up for them, doubtless in the hieratic script, by the priestly colleges. How the workmen's bronze gravers were hardened for the task, and whether they used emery, are questions more easily asked than answered. Anyhow these sculptures deeply cut in the granite are marvels of antique art, especially those *incavo rilievo* hieroglyphs, sometimes seen on these monuments, but only by those who look for the feathers of birds and the like conscientious details wrought out with the utmost finish below the surface of the stone. After all, however, the greatest miracle is the invention of these oldest symbols of speech, and the next greatest is their interpretation after the secret had been so long lost.

The Egyptian obelisks are written and therefore historical monuments in the strictest sense of the term, an immense advantage which they share with the Assyrian, such as the black obelisk from Nimroud in the British Museum, set up by Shalmaneser II., on which the learning and genius of the late Dr. Hincks first read out the record of the tribute paid to that monarch by Jehu, King of Israel. They thus rank far above those Keltic obelisks, called menhirs, and other rude monoliths of a like kind found in India and elsewhere. So long indeed as the key to the decipherment of the hieroglyphics remained lost they were kept out of their birthright, and were like the Pharaoh Anysis, of whom Herodotus writes, driven from his throne to take refuge in the swamps of the Delta. But it should not be forgotten that even during the time of ignorance they pointed like fingers, or like notes of admiration, to the treasures hidden in the stone literature of the Nile. The obelisks brought from Egypt by the Roman Emperors, two of them by Augustus himself, and set up in the capital of the world, were thrown down during the barbarism of the Middle Ages. But after the Revival of Letters they were set up again by the Popes, first and foremost by Sixtus V., who found plenty of work of the kind for his engineer, Fontana. The strange picture-writing on them piqued the curiosity of the learned world, and guessing was rife for two centuries. But the chief gain was that the problem was now fairly posed, and that research could no longer sleep. For the guessers were all on the wrong scent, and there was as yet nothing to set them right. There was a settled belief that the Egyptian hieroglyphs, like the Mexican picture-writing, stood for things, either directly or metaphorically, instead of painting sounds, like the letters of the alphabet. The Jesuit Father Kircher was the great authority on the subject. He had studied Coptic, the latest form of the old Egyptian language, and, on the strength of this accomplishment, was most illogically credited with knowing all about the matter. Hence he was implicitly trusted when he proved by his ideographic translations of the obelisk inscriptions that they revealed the hidden wisdom of Egyptian philosophy and theology, which of course turned out to be miraculous foreshadowings of Tridentine Christianity. It was not until the dawn of our century that the discovery was made which was destined to puff away such airy nothings like the mists before the breath of the morning. The allusion will be understood to that famous Kaaba of all pilgrims to the British Museum, the Rosetta Stone. In 1799, it was found in the ruined temple of the Egyptian god Tum, built by Pharaoh Necho. The finder was M. Boussard, one of the *savants* accompanying the French expedition to Egypt. At the capitulation of Alexandria it was surrendered to General Hutchinson, and was soon afterward presented by George III. to our national collection. The transcendent worth of this black stone lay in the fact that, for the first time, it gave us an Egyptian inscription translated into Greek. The Egyptian is written here in two scripts, viz., the hieroglyphic or monumental, and the demotic or popular, called also enchorial. What is thus engraven in two languages, but in three scripts, is a decree made by a synod of priests at Memphis in honor of King Ptolemy Epiphanes, which is ordered to be inscribed in this threefold form, and set up in all the Egyptian temples of a certain rank. Here, then, was the Archimedean fulcrum found, if this unknown world was to be stirred at all. The Archimedes, who, however, had to be waited for for some time, was the Englishman, Dr. Young, as the illustrious countryman of Champollion, M. Arago, frankly owned in his noble "Eloge" on his compatriot delivered before the French Institute in 1833. On the other hand, none is more keenly alive than our own Dr. Birch, whose sure lead this sketch follows, to Young's halts and shortcomings, after taking the first step. As compared with Champollion, he was but as Newcomen to Watt. Already before the Rosetta Stone changed hands it had been made known and reproduced in drawings by the French *savants*. From the first it was seen that the proper names *could* only be written phonetically, as they are in Chinese, and Silvestre de Sacy pointed out the groups in the demotic inscription which he thought must answer to the oft-recurring name Ptolemy in the Greek. In 1802, the Swedish scholar Akerblad resolved these demotic name-groups into the supposed component letters. Comparisons of this kind were much easier with the demotic, which was three times as complete as the sadly truncated hieroglyphical text. Besides the recurring names Alexander and Alexandria, the demotic presented the group taken to mean king 37 times, and that identified with Ptolemy 14 times. But as to the hieroglyphs, save for guesses by the elder De Guignes, and by Zoega in his great work on obelisks, that after all they might be phonetic, nobody had as yet fairly grappled with them, with the view of testing this hypothesis by the touchstone of the Rosetta. Long after the discovery of that monument, the porch of the temple at Dendera was said to be inscribed with the 100th Psalm, and so late as 1821 the inscription on the Pamphilian obelisk at Rome, now known to have been set up by Domitian, was held to record the victories of the godly over the wicked under the sixth and seventh Pharaohs, B. C. 4000. Meanwhile, Young had approached the philological problem in the spirit of the profound mathematician that he was. He began with the demotic, like the rest, and, unfortunately, he clung to it to the last. But, having been much struck with a suggestion thrown out by Professor Vater, who thought the unknown language of the Rosetta might be resolved into an alphabet of about 30 letters, he studied demotic and hieroglyphic texts alike, from the point of view of the phonetic principle. Having verified the identification of the demotic group for the name Ptolemy, by measuring the relative intervals as compared with the Greek text, he proceeded to test in like manner Zoega's notion that the ovals in the hieroglyphical text contained the same royal name.

The result of applying this mathematical test was that he was now perfectly certain that within that oval was spelt—for it could not be painted—the name Ptolemaios, as in the Greek. He next proceeded to assign to each character within the oval its several function. Here he erred in some details, but in the main he was successful. He afterward analyzed another royal name, which he rightly identified with Berenike. But here also he failed in detail, and Dr. Birch, while admitting that he made out the true value of five characters, adds that "he was unable to analyze by his results more than these two names." In all his other attempts he utterly failed, mistaking *Autokrator* for *Arsinoe*, and *Kaisar* for *Euergetes*, not to speak of other failures, all due to his clinging to the leading strings of the demotic. It was in 1818 that he thus found the key, although it was left for Champollion to turn it more deftly in the rusty lock and to open the door. It was not until three years afterward that the French *savant*, although he had published his "Egypt under the Pharaohs" in 1814, and had served an apprenticeship of full seven years in the research, became acquainted with the new phonetic method of reading the hieroglyphics. But in January, 1822, Mr. W. Bankes, who had already forwarded to Letronne a copy of the Greek inscription on the base of his small obelisk from Philæ, transmitted to the same great scholar a copy of the hieroglyphics on this shaft. These were shown to Champollion, whose genius at once saw in the new materials the means of further verifying and enlarging the English discovery, of which he had by this time become aware. For besides Ptolemy's oval or *cartouche*, as Champollion christened it, there was on the shaft of the obelisk another, which could only be that of Kleopatra, since these two were the royal names found in the Greek below. His parallel analysis of the two was published in his celebrated Letter to M. Dacier, which appeared in September the same year. Prefixing the two *cartouches*, it will be a safe and easy task to follow the bold and firm steps of the master. The queen's name stands before the king's as that which Champollion was the first to read out.

Champollion's comparative analysis of the two names was as follows: 1. The first sign in the name Kleopatra he took to be the knee, the coptic word for which, *keli*, begins, like our English word, with K. The knee stands for K on the principle of our primers, which teach the child A by an apple, and B by a bull. In Ptolemaios there is no K, and accordingly there is no such sign in the second cartouche. 2. The lion, in Coptic *labοi*,. is the second sign in the first cartouche, and the fourth in the second, just as L, the initial of the Coptic word for lion, is the second letter in Kleopatra, and the fourth in Ptolemaios. The Lion, therefore, is indisputably L. Here Champollion corrected Young's rendering of the second cartouche, whose third sign Young treated as silent, while he as strangely read the Lion OLE. 3. The

* A recent lecture before the Society of Arts, London.

third sign in the first oval is the Reed, *akx* in Coptic. In the other oval the Reed doubled stands in the sixth and seventh places, answering to the diphthong AI. In the Kleopatra cartouche it is plainly A or E. 4. The fourth sign is a kind of Noose, which must be the fourth letter, O, in Kleopatra, as it is the third in Ptolemaios, answering to the Noose in the third place of the second cartouche. 5. The fifth sign in the Kleopatra oval, a Mat, is P, the fifth letter in the name and the first in Ptolemaios, in the oval for which the same sign accordingly stands first. 6 and 9. The Eagle occurs twice in the Kleopatra cartouche, not at all in the other. The Coptic word for eagle is *akhoom*, with initial A, and to the eagle in the 6th and 9th places in the queen's oval answers an A in each of the corresponding letters of the name. 7. The seventh sign in the queen's cartouche is the hand, in Coptic *toot*, with initial T, just as T is the seventh letter of her name. This is not the same sign with that which stands for T in the Ptolemy oval. But it was soon found that the Egyptians had more than one T, and so of other letters, just as we have several signs for S, not all of the same origin. One of them, our crotchet S, is plainly a lineal descendant from the S which Young rightly read at the end of the first hieroglyphical word ever deciphered. 8. A mouth, in Coptic *Ro*, fills the eighth place of the first oval, and accordingly its initial, R, is the eighth letter in Kleopatra's name. The ninth and last letter has already been accounted for. But beyond this, the queen's cartouche presents the sign which stands for T in the name Ptolemy, which Champollion here rightly took for the Coptic feminine article, and the Egg, which was also found at the end of other female names, and did not seem essential to the sound. In fact, the egg was afterward proved to belong to the large class of determinative signs, none of which are sounded, as such, although a very few of them (this egg itself happens to be an instance)

now and then discharge a phonetic function too, as syllabic signs. Of these syllabic signs, of which there are about 150, with the determinatives, which number half a hundred more, the long list of hieroglyphs, at which freshmen in the study so often take fright, is numerically all but quite made up. But, after all, the alphabet, as soon settled forever by Champollion, with its fifteen sounds and about twice as many signs, makes ample amends, by its millionfold employ, for the paucity of its letters. It spells not only proper names, but every kind of word, and the syllabic and ideographic signs are dumb until it bids them speak. The Egyptian scribes often pictured a horse simply, and without more ado, to denote that animal, but never before training the reader to associate that picture with its Egyptian name, by habitually spelling that name before it. Of course, when both word and picture were given, the word was not sounded twice; but no less certainly when nothing save the picture was given, it was sounded once. Never was an unfamiliar object written about without both depicting it and alphabetically spelling its name. Of the vast Ygdrasil of Egyptian literature, the alphabet is the root, and all its ramifications Champollion had followed thence before he was laid to rest under his obelisk in Père La Chaise in the spring of 1832. *Requiescat in pace.*

The unraveling of the Egyptian graphic system, that masterpiece of the national art, first made possible something like a restoration of the authentic history of the Pharaohs. But the life of history is chronology, without which it becomes mythical, shadowy, dead. Plato shut the door of his study against all unversed in mathematics. Of the science of the past the time-scale is the mathematics, without which it is not possible to know it aright. Unfortunately the Egyptian time-scale remains to this day hardly less the plaything of subjectivity than was the problem of hieroglyphical decipherment before the time of Young and Champollion. You need not be reminded of the thousands of years that yawn between the extreme dates given still for Menes, the founder of the Pharaonic monarchy; nor of the many hundreds between those for Usertasen I., whose obelisk, still standing at Heliopolis, is by far the oldest of the colossal order known; nor of the fewer centuries, but still quite enough to bewilder earnest students, between those for the Thothmes and Ramses, who have written on our Needle their own glorious names.

The second generation of hieroglyphists is passing away, and that is where we are at present. But let us hope on. We have seen how the mutual light shed on each other by the Rosetta Stone and one of the lesser obelisks cleared up the darkness in the one instance. What if it should be reserved for the same bilingual monument, by enabling us to understand an all-important note of time found on several of the greater obelisks, to break at last the secret of the lost chronology, as it before loosed the string of the dumb Egyptian tongue? It has taught us to read. It may, perhaps, teach us to cipher.

Of these greater obelisks Professor Erasmus Wilson—to whose genial pen, as well as to his open heart and purse, all interested in these monoliths are so deeply indebted—reckons we have a dozen left. He includes in this class all 50 ft. high or more. The line may be somewhat arbitrarily drawn, but it may be worth remarking that those he enumerates are wont to be styled "great" in their own inscriptions. Five are still in Egypt, viz., two at Karnak, and one each at Luxor, Alexandria, and Heliopolis. Four adorn Rome. These are the Lateran, the Vatican, the Flaminian, and the Campensis, as Ungarelli calls that of Psammetichus II., which Augustus erected in the Campus Martius, and which now stands in the Piazza di Monte Citorio. The other three belong, respectively, to Constantinople, Paris, and London. Since there are two pairs among the dozen, viz., the Paris obelisk, with its widowed mate at Luxor, and the two Cleopatra's Needles, they represent but ten distinct erections. Now, according to the inscriptions, four of these ten erections took place on the First Festival of some one or other Thirty-year cycle. Usertasen's, at Heliopolis, Queen Hatasu's, at Karnak, our own Needle, and the Campensis, at Rome, are all thus dated. Of a fifth erection, that of the Flaminian obelisk, judging from other inscriptions, as well as its own, the same thing is morally certain. Thus half of our dozen colossal obelisks are strictly chronological monuments, and the erections—unless any two or more were coincident in time at least, although not, it may be, in place—must have occurred at intervals of just 30 years, or of multiples of that number of years. Yet, but for the Rosetta Stone, we should never have known this interesting and important fact. Its interpretation of the chronological notation which we find on these great obelisks, as well as on other Pharaonic monuments, is our only warrant for connecting it with recurring periods of thirty years, or trentenary cycles. There is here no hieroglyphic for either "years" or "thirty," but only for festivals of a certain kind, called, in Old Egyptian, *hibu set*, "festivals of the *set*." By this word *set* has been understood intercalation; *e. g.*, by Brugsch Bey, who formerly took them to have been those of a day every fourth year, as in our leap years. He has lately abandoned this view in favor of the hypothesis started by his learned countryman, Dr. Gensler, of Weimar, who, though without the least attempt at proof, assumes the trentenary cycle to have been a luni-solar one, with eleven intercalations in all, each of a lunar month.

I too think *set* here means intercalation, and that there were eleven such in each cycle, or very exceptionally twelve. This latter number I have found mentioned in a festival list inscribed, under the Twelfth Dynasty, in the tomb of Chnumhotep at Benihassan. At Silsilis are recorded all the intercalations belonging to one cycle, from the first, which took place in the 30th year of Ramses II., to the sixth, which fell in his 45th. In an Anastasia papyrus another, which must have been the ninth of the same series, is dated on the 26th of Mechir, in his 52d year. This evidence strongly supports the conclusion that there were eleven intercalations in all.

But so there are eleven intercalations—of a day, however, not a month—in a modern trentenary cycle, which deserves to be compared with the ancient, and which Lepsius, in discussing the latter, about such a cycle ago, when he wrote his great work on the "Chronology of the Egyptians," did not forget to mention. So far he may be held responsible for my rushing in where he did not himself dare to tread. I venture to maintain that "the thirty-year cycles," of which, on the Rosetta Stone, Ptolemy Epiphanes is said to be "lord, like Ptah, the great," are still in use all over the Moslem world. The Hejira starts from the first day of one of them. But, as Ideler long since proved in his "Handbook of Mathematical and Technical Chronology," they were introduced in Arabia ages before Mohammed was born. He would not have erred had he said several thousand years. There, in the Sinaitic peninsula, Egyptian monuments of the Sixth Dynasty vindicate for them an antiquity stretching back to the fourth millennium before the Christian era. The cycle consists of thirty free lunar years, as Ideler styles them, of which eleven—viz., the 2d, 5th, 7th, 10th, 13th, 16th, 18th, 21st, 24th, 26th, and 29th have an intercalary day added at the end to the other 354 days. Thus the cycle numbers $354 \times 30 + 11$ days, or 10631. This simple device of the elevenfold intercalation of a day is to cover the excess of the lunation over 29½ days; and so well does it perform its function of keeping the kalendar moon true to the heavens, that the error does not exceed a day in, say, the space of time that has elapsed since the Christian reckoning began. The ancient Egyptians, as we have seen, kept these intercalations as festivals, the *hibu set*, and I have observed that the Pharaoh's coronation was always solemnized on the Neomenia thus inaugurated, *i. e.*, on the morrow of the intercalation.

It is now fifteen years ago since I first noticed the striking fact that, between the hieroglyphical dates, recorded in the Sinaitic peninsula, of the First Intercalation Festival of a Thirty-year Cycle, on the 27th of the eleventh month in the 18th year of Pepi I., a Pharaoh of the Sixth Dynasty,* and the First Intercalation Festival on the 3d of the second month, in the 2d year of the king, identified by Baron Bunsen with Mentuhotep II., of the same dynasty,† the interval is exactly six such cycles as the Moslems use, or 6×10631 days. The calculation takes account of the otherwise attested state of the Egyptian civil kalendar at that early epoch, which had then but twelve months of 30 days each, or 360 days in all, the five *epagomenæ*, or "added" days at the end, not having been introduced before the Twelfth Dynasty. The years of Pepi, or Phiops I. and Mentuhotep II., were still what my learned and lamented friend, the late Dr. Hincks, who took a deep interest in my researches, and agreed with me in the main as to this thirty-year cycle, was wont to call anepagomenic.

With this interval of just six Moslem cycles between the two Pharaonic dates tallied the interval recorded by Manetho's existing text, as checked and rectified by Eratosthenes on the one hand, and by the fragments of the Turin Pharaonological Papyrus on the other. The comparative criticism brought to light a deficit of a couple of reigns, one of ten and the other of twenty years, in our present copies of the Egyptian priest's list of the kings of this dynasty. It should be added, however, that one copy makes the kings eight in number instead of six, which so far confirmed my criticism. Shortly afterward it struck me that 1,447 anepagomenic years contained but one day more than 49 of the Moslem cycles, so that by assigning a twelfth intercalation to one of them, say the 25th for symmetry, I could make another chronological experiment. This was to tabulate an anepagomenic period, beginning with the first day of a first trentenary cycle, and ending, on the 360th day of the 1,447th year of the period, with the last day of the 49th such cycle. Of course all I cared to mark in my table was the month and day in the anepagomenic kalendar, on which, according to the Moslem rule, the first intercalation in each thirty-year cycle would fall, so that the results might be compared with the Sinaitic dates. Here is the table:

Table of the Anepagomenic Period of 1,447 years of 360 days, showing the day of such year on which the First Intercalation of each of the 49 Thirty-year Cycles therein contained would fall.

I.—xii.	19	XIII.—v.	1
II.—vi.	30	XIV.—xi.	12
III.—i.	11	XV.—v.	23
IV.—vii.	22	XVI.—xii.	4
V.—ii.	3	XVII.—vi.	15
VI.—viii.	14	XVIII.—xii.	26
VII.—ii.	25	XIX.—vii.	7
VIII.—ix.	6	XX.—i.	18
IX.—iii.	17	XXI.—vii.	29
X.—ix.	28	XXII.—ii.	10
XI.—iv.	9	XXIII.—viii.	21
XII.—x.	20	XXIV.—iii.	2
XXV.—ix.	13	XXXVIII.—viii.	7
XXVI.—iii.	25	XXXIX.—ii.	18
XXVII.—x.	6	XL.—viii.	29
XXVIII.—iv.	17	XLI.—iii.	10
XXIX.—x.	28	XLII.—ix.	21
XXX.—v.	9	XLIII.—v.	2
XXXI.—xi.	20	XLIV.—x.	13
XXXII.—vi.	1	XLV.—iv.	24
XXXIII.—xii.	12	XLVI.—xi.	5
XXXIV.—vi.	23	XLVII.—v.	16
XXXV.—i.	4	XLVIII.—xi.	27
XXXVI.—vii.	15	XLIX.—v.	8
XXXVII.—i.	26		

* Lepsius, Denkmäler, II., 115.

† *Id. ib.*, II., 149, c.

I own that, although the chances were more than 7 to 1 against me, had accident only been concerned I was fully prepared to see the hieroglyphical dates turn up in the table, as is found to be the case. But, to my amazement, the Phiops monumental First Intercalation came to light as that of the last cycle but one, and the Mentuhotep date accordingly for the fifth cycle of a new period. This showed that if, as could hardly be doubted under the circumstances, the Anepagomenic Period was in use among the ancient Egyptians, it must have been already known to them about fourteen centuries at least before Mentuhotep's time, which carries us back beyond Menes. The first person to whom I made known these results—the year was 1863, and the place the library of the British Museum, with these hieroglyphical texts in Lepsius' great work open before us—was the profound Egyptologist who has so lately passed away, Charles Wycliffe Goodwin. The linguistic, rather than the chronological, research was his line. But he was fully alive to the great importance of the latter, and strongly urged me to persevere. Our acknowledged master in the science, Dr Birch, with a like genial appreciation of an enthusiasm not his own, did the same.

The nature of the Egyptian Thirty-year Cycle the crucial experiments on the Sinaitic dates seemed to make pretty clear. It was the *alter ego* of the Moslem cycle of thirty times twelve moons. Subsequent research convinced me that this oldest form of the Pharaonic trentenary cycle—for, when it had to be grafted on Amenemha I.'s new kalendar of 365 days, its epoch was shifted some months, not to speak of other changes—was virtually the *idem ego* of the Arab reckoning. If it be so, there surely was never a more startling illustration of the proverbial immobility of the East. It would seem that the lunar computation current at this hour in the mosques must be at least verging toward the eighth millenary of its age. But your time and kind attention must not be taxed with the details of the solution of the terrible problem which was still behind—that of the recovery of the lost Anepagomenic Kalendar, of which one might well have despaired. For hopelessly lost it seemed to be at first, although I have since found its New Year's day directly equated with a monumental date of our great obelisk Pharaoh, Thothmes III. The date in question is the 21st of the month Pharmuthi, in the king's second year, answering to the 23d of April, B. C. 1513. This was found to square exactly with the conclusion, reached by a quite independent road, that the Sinaitic monumental date of Phiops I. answered to the 9th of March, B. C. 3568, and, consequently, that of Mentuhotep II. to the 28th of October, B. C. 3394. From the latter date, it may be of interest to observe, in passing, there are but a dozen years to the end of the Sixth Dynasty. There the Turin Pharaonological Papyrus gives a total sum of [1] 355 years from Egypt's protomonarch, Menes, when we restore the missing 1000 of which a hole in the fragment has robbed us. According to this, his accession was B. C. 4736. By extending far enough downward the table of the Anepagomenic Period, from the epoch just given for the First Intercalation Festival, in the reign of Mentuhotep II., its 45th cycle will be found to coincide fairly enough with that from which the Hejira starts, on June 15–16, A. D. 622.

Let me now return to our Needle, on which, in October last, Dr. Birch first read out from Burton's faulty text the important trentenary date, since verified by a photograph, on which it was set up by Thothmes III. The three distinct but convergent lines of astronomical evidence for my chronology of that Pharaoh's reign are developed in an Appendix, written at the request of the Chevalier Ernst von Bunsen, and published in his "Chronology of the Bible" four years ago. On this evidence ultimately rests my daring identification* of the day of the double erection of Queen Hatasu's pair of obelisks at Thebes, and her brother's Heliopolitan pair with the 28th of August, B.C. 1502. It was the First Intercalation Festival of the Cycle. How now stands the reckoning in the rear from the like festival under Mentuhotep II.? Measure off 65 cycles from the 28th of October, B.C. 3394, and the line strikes the year. If it fails to hit the month it is because of the shifting of the epoch of which I have already spoken. To test the calculus in front, let us start from the era of Menophres, mentioned by the Alexandrian astronomer, Theon. According to Dr. Hincks' rectification, it begins with the heliacal rising of Sirius, or the Dog star, on the Egyptian New Year's day, 20th July, B.C. 1323. Dr. Hincks, moreover, exploded Champollion-Figeac's premature identification of this important Pharaoh with Menephtha, the son of Sesostris, or Ramses II., to whose grandfather, Menophres Ramses, who reigned but 16 months, he further proved it to belong. Thence it is but a step, the 54 years' reign of Sesostris' father, Seti, to the reign of Sesostris himself, beginning B.C. 1269. Within the few months of the joint rule of father and son falls the date of the famous Quatercentenary Stela found at San, the ancient Tanis or Zoan, the old Hykshos stronghold. Accordingly, it reckons by the Hykshos kalendar, falsely attributed to Augustus, who is thought to have introduced it, along with his Alexandrian era of Actium, which begins with its New Year's day, the 30th of August, B.C. 30. But no ancient author tells us this, and the alleged coincidence, in that year, of this New Year's day of the fixed Egyptian year of 365¼ days with that of the wandering year of 365 days only, which is the solitary argument adduced to prove Augustus' claim, did not take place, as Ideler has pointed out, until five years afterward. In point of fact, the first of the Cæsars had already, under the guidance of the Egyptian astronomer Sosigenes, made this fixed kalendar the model on which he framed the reform Roman kalendar named, after him, the Julian. He even, as my friend Professor Lauth, of Munich, has solidly demonstrated, borrowed bodily from this original, Bissextile day, which was actually common to both. I have ventured to term the invention Hykshos on the strength of a fragment from Manetho, preserved for us by the scholiast on Plato, which attributes to Salatis, the Shepherd King who conquered Egypt, the development of precisely such a kalendar, out of a previously existing lunar one, which, however, would not on that ac-

* In the *Graphic* newspaper of Feb. 2.

count be necessarily laid aside, especially by a Semitic race, such as these Edomite conquerors were. Salatis added, we are told, 12 hours to each month, and six days to the year, so that there were 366 days in all, as there would be in the first year of a bissextile reckoning. In the contribution, already mentioned, to the Chevalier Bunsen's work I have given my reasons for believing that the Hykshos conquest of Egypt took place B.C. 2086. I hope, therefore, I may be forgiven if I see in the remarkable fact that on the 30th of August in that year, the New Year's day of the fixed kalendar, began the new Anepagomenic Period next to that under which the reign of Mentuhotep II. falls, a strong circumstantial corroboration of Manetho's testimony as to the introduction of that kalendar.

Accordingly, I understand by the 4th of Mesori, on the San Stela of the 400th year of the Hykshos, King Sethos, which we already know to have been the first year of Ramses II., and the last year of his father, the 28th of July, B.C. 1269. The stela presents a slight indication, which has accordingly been too often overlooked, that this was the day of one of the eleven intercalations of a thirty-year cycle. This can only have been the first of the eleven, since several monuments make the First Festival of the Intercalation fall in the Pharaoh's 30th year, so that the same must have fallen in his first also, or B.C. 1269. Measuring forward now eight trentenary cycles from the date assigned for the erection of our Needles to the San date, the line again strikes the year, but no more. The reason is as before. The San date belongs to the archaic series of cycles, and if we want an exact hit we must measure off 73 cycles from the First Intercalation Festival under Mentuhotep II., the 28th of October, B.C. 3394, and then we shall find the line striking the Edomite date, at the old Hykshos city, year for year, month for month, and day for day.

Of the almost certain fact that to this same first year of Sesostris belongs the Flaminian obelisk at Rome, with but one of its medial columns engraven by that king, while those of the other three sides were cut by his father, I have already spoken.

It is a pity that no one of the inscriptions which mention the Festival of the First Intercalation in the 30th of Ramses II. gives us the month and day, so that the testing computation of the intervening nine cycles between this date and that assigned to our Needles might have been exact. But we may well put up with the next best thing. In the course of this paper an Intercalation Festival, presumably the ninth of the eleven, which fell on the 26th of Mechir, in the 53d year of his reign, has been cited. Here then is a fairly stringent test of my chronology, which gave the 28th of August, B.C. 1503, for the twofold obelisk erection. The Ramses date reduced is the 16th of December, B.C. 1219. How then stands the reckoning? In lunar time the interval is just nine thirty-year cycles and twenty-two free lunar years to a day. Now, according to the current Moslem rule, if the former date was that of the first intercalation, the latter would be that of the ninth. It will seen the sum works.

THE ART OF MARBLING.*

By C. W. WOOLNOUGH.

THE art of marbling, as applied to paper for bookbinding and for the ornamentation of book edges, is one of which, in the general sense of the term, very little is known and very little is recorded. Its origin and antecedents are involved in obscurity, and I have sought in vain for reliable information on this point.

Having seen some marbled paper on books bearing dates of a couple of centuries or more ago, I once thought it probable that might have been the time when it was first brought into use; but, as it is very possible that these books might have been either re-bound or repaired at a more recent date, when the marbled paper was applied, I gave up that idea as one on which no reliance could be placed. I was told many years ago, by an old man, that marbled paper of the old Dutch pattern used to be imported into this country from Holland; and in order to evade payment of the duty, which was rather heavy, sheets of it were wrapped round small packages of toys, and thus passed free, these sheets being afterward smoothed out, pressed, and sold at a highly remunerative price to the bookbinders; and I have occasionally met with some of these ancient specimens, which retained a marvelous softness and brilliancy of color, and displayed a considerable amount of skill in their manipulation.

To any one seeing the process for the first time it appears to be very easy; indeed I have heard many people observe, "Oh, any fool could do that, if they did but know how to mix up the colors." I am quite willing to admit this, but to mix the colors, and to keep them in good working order, is not quite so easy after all, as the chemical changes which are constantly taking place and the influences of the atmosphere will speedily prove.

I will now endeavor to tell you what the process is.

Marbling is the art of producing certain patterns or figures, by the means of colors so prepared as to float upon a surface of mucilage. Although several colors may be thrown, sprinkled, or laid on together, or one after another, yet they will still each retain a distinct position, and will not mix either with each other or with the vehicle on which they float; and thus, while floating, they may be formed into the desired pattern before being transferred to the paper, which is accomplished by gently and carefully laying the sheet of paper down gradually upon the floating colors, which will instantly adhere to the paper, and, when lifted out on a rod or lath, will leave the surface of the mucilage free for a repetition of the process.

Now we must observe that this is a very different thing to the manufacture of room or wall-papers, which is usually designated by the name of paper-staining; this is all done with blocks of wood, cut and carved out by hand, and is, in fact, a kind of printing, so that an exact repetition of the same device can be produced *ad libitum;* whereas, in this process, the effects being natural, no two can be found exactly alike, though called by the same name and passing for the same pattern; nor could any artist, with all his skill, produce a fac-simile of what may be accomplished by this method in a few minutes, were he to try for a whole month, or longer. Now, there are two questions which we will take into our consideration this evening, and the first is, "What is it that causes the color to float and spread out upon the surface of mucilage?" and, secondly, "What is it that prevents the colors from commingling or running into each other when they fall one upon another?" What is it that keeps each color clear and distinct, though several may be thrown on at the same time, and though they may be even twisted round with a piece of wire or pointed stick?—as I shall show you presently. There are mysteries in all the operations of nature, some of which are very beautiful, and interesting to an inquiring mind. The great Michael Faraday himself, in one of his letters to me, observes, in reference to this art, "I feel much interest in the subject, not only on account of its associations with my early occupation of bookbinding, but also on account of the beautiful principles of natural philosophy which it involves;" so, however it may be depised, if it was not depised by, but was an object of interest to, that great and good man, surely it cannot be beneath our notice either. I will now endeavor, in a plain and simple way, to explain to you the so-called mysteries of this process, which some have so long and so jealously concealed from the vulgar gaze, and I verily believe that its extreme simplicity was the principal cause of the extreme watchfulness maintained by those who then practiced it. Fifty years ago it was almost as difficult to get a sight of the inside of a marbling establishment as it would be to get into the presence of royalty; every crack and aperture, nay, even the very keyholes were stopped up or obscured, to prevent any glimpse being obtained as to the method by which it was accomplished, and, as comparatively few were in possession of its secrets, it was a very remunerative craft, and good profits were realized.

When I was about 13 years of age, I accompanied an individual who was going to fetch some books which had been sent out to be marbled; when we arrived they were not all finished, and your humble servant was admitted into the sanctum to wait for the remainder. I was so stricken with wonder and admiration at the sight, that I resolved in my own mind not to rest till I had found out how to do it. I will not now take up time by telling you of all the failures and the disappointments I met with; but at length, by unremitting toil and persevering efforts, I was in the end rewarded with success, and though for a long time the results were very imperfect, yet I could perceive that I had got hold of the root of the matter, and, after much practice, I of course approached nearer a perfect result; and as I was indebted to no one for the knowledge I had obtained, in the year 1853 I published a small work on the subject, which gave great offense to the fraternity on account of its truthfulness and the way in which the various kinds of marbling were set forth and the manner of their accomplishment explained. You see here now before me a shallow vessel or trough, about 2½ or 3 inches in depth; it contains a thin mucilage. Now, there are several kinds of mucilage, viz., first and most important, gum tragacanth, called by some gum dragon; secondly, linseed or flax seed; thirdly, Irish or Carrageen moss; fourthly, a seed, called, among the initiated, flea seed, the name being given on account of its great resemblance to that well-known but troublesome insect, but really the seed of a kind of plantago, cultivated, I believe, in the south of France, and much superior to linseed, on the ground that it retains its properties a much longer time; besides, one tablespoonful will produce as much mucilage as half a pint of the linseed. The mucilage used this evening is a solution of gum tragacanth. It requires two or three days to dissolve, and requires to be frequently stirred or beaten up during that time, in order to break the lumps and to combine the gum with the water, after which it must be strained through a fine hair or muslin sieve before using. I shall now proceed to show you by what means the color is made to float and expand upon the surface of this liquid, but first will prove that without some other agency than water in the mixing of the colors they will neither float nor spread, and there is but one thing at present known that will effect this. It is a very simple though subtle and powerful fluid—no expensive or elaborately prepared article; it is purely a production of nature, being nothing more or less than the gall of an animal. Ox-gall is the sort generally used; it is the easiest to be obtained, and is within the reach of all who may wish to try the experiment. I will not say that the gall of a horse may not be as effective as that of an ox, having never tried it; but I have tried the gall of sheep, and proved it much weaker, so that ox-galls diluted with water would be quite as good as that; but there is sometimes a great difference in the galls taken from different animals, some being far more powerful in their operation than others. The gall of one animal may be thick and ropy, which is objectionable, while that from another may be beautifully fluid, which is more pleasant to use. When fresh it has no unpleasant odor, but when stale it is anything but agreeable, though none the worse for the required purpose; indeed, I consider it preferable.

I will now proceed to show you the action of the gall. I first draw this thin flat piece of wood, called a skimmer, over the surface of the solution trough. You ask me why I do this. I reply, because while I have been reading or speaking there has been forming on the top of the liquid a film or thin skin which, though imperceptible to the eye, is quite sufficient to frustrate the proper result. I now take a color mixed only with water and allow a few drops to fall; it neither floats nor spreads, but falls to the bottom of the trough. I now pour a little gall into the same color and again sprinkle some on the surface; this time, however, it floats and spreads all over the surface of the mucilage. I just lay on that a piece of white paper, and pouring into a glass a little water, I add a few drops of gall, and with a brush sprinkle it over the portion of the surface of color not covered by the paper, and you will see that it has divided the color into veins, thereby showing the effect produced by the gall when it is mixed with the colors; and in order to enable you to see the effect of this more vividly, I will lay on another piece of paper by the side of the first, and lifting the two out simultaneously, the operation or effect of the gall is clearly made manifest. This is the foundation of the whole process; this is the root from which all the branches spring, and although there may be other ingredients required to produce various effects, still these two simple productions of nature—the gum and the gall—constitute the life and soul of this (as the late Dr. Normandy designated it) "pretty mysterious art."

In order to exemplify this more fully, permit me to give you another illustration, which I think will satisfy you as to the correctness of my statements. Take five different colors, viz., red, black, orange, blue and buff. The first color, red, is mixed with a small proportion of gall in it; the second with a little more; the third, orange, with more still; and so on, each succeeding color requiring additional proportions of this fluid, in order to enable it to find a place for itself by displacing or pushing aside the previous colors, and driving them up into a smaller compass, thereby rendering them more intense and solid, and better adapted for their formation into such devices as the operator may desire.

First, I will apply the red; it spreads out over almost the whole surface of the solution, so that you can hardly perceive it, but I will, nevertheless, just lay down a small piece of paper after each color, that you may see the effect more definitely when transferred to the paper than you can possibly do as it floats on the trough. Next comes the black. Thirdly, the orange; this is very plainly seen as it falls. Fourthly, the blue. And lastly, the buff. These comprise all the colors required for the production of some well-known kinds of papers, but still the pattern is not complete, and I shall have to repeat the process I have just gone through, and continue the manipulations from the point at which we left off, till something like a definite result is obtained. As I observed before, Dame Nature is a very fantastic creature to have to deal with, and the further we go into the subject the stronger will be the proofs of this statement.

Now, after laying on all the colors, it rests entirely with the operator what shall be produced after all, for out of these few colors, as they now lie floating on the surface, a diversity of results can be obtained quite distinct the one from the other, and with a piece of pointed stick, a comb, or a piece of wire, you may indulge your fancy to almost any extent; and then, by diversifying the colors or the arrangement of the colors, an almost infinite variety of combinations and changes may be produced. Having disposed of the first question, viz., What is it that causes the color to float and spread out upon the surface of the solution? we come to the second, What is it that keeps the colors from commingling, when they fall one upon the other? or in other words, what is it that keeps each color clear and distinct, though several may even be thrown on at the same time, and though they may be even twisted round, nay, almost stirred with a pointed stick, every color retaining its perfect individuality, though in so very minute a degree as to require a magnifying glass to reveal it? My humble opinion is this: The moment the color touches the surface of the mucilage, it displaces a portion of mucilage itself, which forms a kind of bulwark or barrier around it, and thus prevents it meeting with the colors which had been previously put on, so that, were you to put on a yellow and immediately follow with a blue, there would be no signs of a green being produced, although if you were to mix the two colors together before you put them on to the mucilage you would have a decided green at once.

I have no intention of entering into a discussion upon acids and alkalies, chemical affinities and combinations, and do not profess to understand them, except in their action upon the materials connected with my present subject, as I think it better, therefore, to confess my ignorance at once on this point than to occupy your time by pretending to explain anything beyond my power. This I know, that the more potent acids, such as sulphuric, nitric, muriatic, etc., would disarrange, nay, destroy, every attempt to produce the results I have just now exhibited or may attempt to produce this evening. I believe acids are used in some of the cheap papers imported into this country from the Continent; but they find but little favor among competent judges of the art. A little alkali, however, is sometimes useful to correct the acidity in some of the colors, when they have not been sufficiently washed in the making, as also to soften the hardness of the water used in the preparation of the mucilage; but I will pass on to another peculiar feature in this process, and that is the very remarkable effect produced by a slight motion of the hand of the operator, while he is in the act of laying the paper down upon the color as it floats, without the aid of any instrument whatever. This class of marbling has been discovered and introduced but little more than half a century, and was in reality quite a new feature in the art. It succeeded amazingly, obtained high prices, and an almost unprecedented demand, which continued for several years, until, as more people got into the way of doing it, and more expeditious movements were attained, it came into such general use that the public got tired of it, and it is now seldom used, except on work of a lower standard. The name given to it was Spanish; we are not to suppose by this that the name has any reference to its nationality, but simply to distinguish it from other kinds then in use, as French, Italian, Dutch, &c. Various stories are told concerning the way by which it was first discovered, some of them being ridiculous enough. I will just allude to two of them. The first is as follows:—One man was intently engaged on his work, and had all his colors laid on; just as he was on the point of laying down the sheet of paper, some one drove violently against him or the trough, by which the whole surface was agitated and set in motion like the waves of the sea, producing an effect which excited further inquiry and study, resulting in the production of this very pretty description of marbling. I have also been informed that the first that was made was produced in the following manner: When all was prepared for the laying on of the papers, one man got under the trough and shook it, so as to produce a wavy motion, when the paper was instantly applied by another, producing the wave-like appearance; these were, however, so broad and irregular, when compared with what is done by the present method, besides occupying two to do the work of one, that it fell into disuse as soon as the improved method was brought to light. Another story is this, and I am sorry to say that there is a considerable probability of there being some truth in it, as that bane of society, strong drink, is indulged in to excess by many of those who are engaged in this calling: One of these unfortunates, with trembling hand and shattered nerves, went to his employment one morning, after a bout of drinking, driven, I suppose, by necessity; and when he came to lay the paper down his palsied hand shook so much that he spoiled, as he admitted, every sheet of paper he attempted to make. Some of these attracted the attention of the principal, to whom the cause was explained, and the light thrown on the subject gave rise to further investigation, till at last the perfect development was obtained. I do not vouch for the truth of these narratives. I give them to you just as I received them, and hold myself in no way responsible for their veracity, but leave you to form your own opinion and judge for yourselves. But I must pass over some very interesting varieties, as I would not trespass beyond the usual bounds. I should like, however, to show you the way in which this Spanish is varied, which I think will interest you when you see how trifling a thing makes a great difference. In this experiment I will produce on one piece of paper the lights and shadows in diagonal lines, while on the other they will be shaded something like watered silk.

There is one more variety to illustrate, of quite a different character to any of the former; in this several colors are put on at once without the aid of brushes, and by this method a considerable degree of uniformity can be obtained. It is one of the oldest styles of work revived and modernized, and is in great demand at the present time for antique and the better class of binding. It is called old Dutch, and consists of several varieties, some large, some small, some curled some not, but all, so far as the laying on of the colors is concerned, conducted on the same principle, the varieties being produced by the manipulations after the colors have been laid on.

Now, if you will attentively observe a whole sheet of this class, you will perceive that the colors are not scattered

* A recent lecture delivered before the Society of Arts, London.

promiscuously over the whole, but follow each other in a kind of regular succession, in a diagonal direction across the sheet, red being the preponderating color. In order to accomplish this, a number of little pots, or tins, are required, about 1½ in. or 2 ins. wide and 2 ins. or 3 ins. deep. Small jam pots will do very well. You will also require two frames, fitted with wooden pegs, and placed at regular distances apart—about four or more inches—having the appearance of a farmer's harrow in miniature. The frames of pegs must correspond with each other in every respect, so that, if you made an impression with one frame on a sheet of paper, the other ought to fit exactly upon the impressions produced by the first; because the colors you will have to apply with the second frame will be placed exactly in the center of the color put on with the first.

The pots must now be arranged in two divisions, an equal number in each, and adjusted so as the teeth or pegs of the frames will drop in the center of each pot, as you will have to give a motion to the frame to stir the colors, as they soon settle; one of these divisions of pots must be half filled with white or ground pipeclay, the other with three different colors, arranged in the following order, the number varying according to the size of your paper. Y stands for yellow, B blue, G green:

G	Y	G	Y	G	Y
Y	B	Y	B	Y	B
G	Y	G	Y	G	Y
Y	B	Y	B	Y	B
G	Y	G	Y	G	Y

Instead, however, of having pots for the white, you may have a trough or vessel the size of your frame, about three inches deep, for the reception of that color, which will answer the purpose equally well and with less trouble.

The red, which is the first color to be applied, must be sprinkled on with a brush, and the surface well covered; then lift carefully the first frame, consisting of the white, giving it a rotary motion so as to stir up the mixture, and let the extremities of the pegs with the color on them just touch the surface of the mucilage in every part; put it back in the color, and quickly take the other charged with the three colors, and in like manner let that touch just in the middle of the spots of white; then with a tapering piece of wood—the handle of a brush for example—draw the colors in a parallel direction up and down, from front to back, after which draw the comb through the color, from left to right, and the pattern is complete, unless you think fit to add curls or any other device, which, of course, must be left to your own discretion.

Thus far we have gone without the aid of any other agent in the colors than gall, and there are many more varieties to be produced by the same material. There are also some very pleasing results to be obtained by the use of other agencies, but it is impossible to compress them into the compass of one single paper. I could have enlarged on this part of the subject to-night, and have mentioned many strange and interesting facts regarding this art. What I have so imperfectly revealed to-night has, I hope, proved that there are many things in everyday life which escape our notice, simply because they are common, but from which we might draw much that both interest and enlighten us, if we would but exercise the powers with which God has invested us, and placed us so far above the inferior creation. With one more experiment I will now close this paper, and that will simply be to show that whenever the paper is wetted with the solution, no color will adhere to it while the moisture remains on its surface. I will now wet some part of the paper, and, after preparing a surface of color on the trough, will lay on this sheet of paper, and, on lifting it out, you will see that the part wetted will be bare of color, while the part that remained dry is perfectly marbled.

The various methods of producing the different descriptions of marble paper were illustrated by Mr. Woolnough, who made a number of specimens in the room. Examples of the printed paper were also exhibited, as well as specimens of the materials employed.

PRESERVATION OF WOOD.

By J. CLARK JEFFERSON, A.R.S.M.

ALL timber which is intended for immediate use in mines must be barked, otherwise rotting would soon commence beneath the bark, and spread among the timber. For this reason leaf wood should always be barked; it is, however, not of so much importance in the case of pine, in which the rotting, owing to the great amount of turpentine, spreads much less rapidly. According to Lottner the experience has been obtained in the Planitzer coal mines that where the pits are very dry, and a fungus soon appears, unbarked pine lasts four to five times as long as the most perfectly dried; it appears as if in green wood the rot proceeds from the center to the outside, while the contrary is the case with dry wood.

Timber is kept stored either under sheds or in the open air; where there is any very large quantity the latter method will be that most usually adopted. When kept in sheds the sides of such sheds are always left open, so that while the wood is preserved from sun and rain it is constantly exposed to a current of fresh air. For this latter reason the wood should never be piled close together, but always so that fresh air can circulate among it; and also for this same reason timber is usually laid alternately crosswise, and when in large quantities is often re-piled. The bottom of a pile should never rest directly on the ground, which might be damp; it is necessary, therefore, to have the timber yard properly drained. Wood which has been cut should always be kept under sheds; timber intended for use as pipes, and which it is intended to bore out, should not be subject to any drying whatever, but should be preserved under water, by which little risk is run of the timber becoming cracked.

The measuring of timber is effected sometimes simply according to weight; when the pieces are all one size or shape, according to number and according to cubic capacity, either by measuring the length and taking the mean diameter at the center of the length, or by measuring three diameters (at each end and in the center), and the length from which the cubic contents are calculated by the use of tables.

Good timber can readily be judged of by the appearance of a freshly cut cross section. A dark color, if uniform over the cut surface, is indicative of strength and durability; if of a dull chalky appearance, of bad timber. A rotting core will have a darker appearance than the rest, and the surface will appear woolly and feel rough. A good timber will not readily clog the teeth of a saw with loose fibers, and the freshly cut surface should feel smooth, and have a translucent appearance; the annular year rings will be close and fine, and the wood when struck on a cross (to the axis of the stem) section with the head of an ax will have a clear sound, and, lastly, will have straight growth.

The durability of timber in a mine is generally put an end to either by being broken by pressure or destroyed by rotting. The destruction of timber by rotting takes place either by dry rot or by wet rot.

Timber which is confined in a place where there is little or no ventilation and an absence of moisture decays by dry rot, which appears to attack the whole of the timber uniformly and simultaneously, accompanied with the formation of a fungus, and the timber is finally converted into a white powder, accompanied by a peculiar sharp sweet smell. Dry rot attacks timber much more quickly than damp rot, and often completely spoils the wood within six weeks. Damp rot generally commences at knots or other irregularities in the wood, and spreads from these to the rest of the timber. In the presence of a considerable amount of moisture the outside surface of the wood becomes slimy and slippery with the formation of fungi often of considerable dimensions, which are sometimes fibrous and sometimes spongy.

Both dry and damp rot are infectious; it is, therefore, advisable for the preservation of adjoining timber to remove that which has been attacked. Damp rot mostly occurs in places subject to alternations of moisture and dryness, heat and cold, and in stagnant atmospheres, and, therefore, at the mouths of levels, day holes, at pit heads, and other places exposed to the variations of the weather and the seasons, in the distant unventilated parts of a mine, in the places exposed to the return current sooner than in places exposed to the intake current, and in the upper parts of shafts near the surface.

According to experience, the proximity of clay or clayey strata is said to lead to a rotting of the adjoining timber sooner than other kinds of strata, and those sides of timber exposed to the air currents are said to be less liable to rot, and rot less rapidly, than the side from the air currents.

The durability of timber depends, therefore, as is evident, on local conditions. In uniformly moist places the oak will often last 40 years, and fir and pine 15 years, before it is necessary to take them out and replace them. According to Ottilice, in the brown coal mines of Saxony, except in the case of extra weight or pressure, pine and deal last four years; according to Gatzschmann, in metalliferous mines the timbering lasts from five to six years, but under unfavorable circumstances from one to three years only, in coal mines often not longer than six weeks; under favorable circumstances—when kept wet, cold, and in a good air current—from 20 to 30 years, and even more; as at the Sauberg Mine, near Ehrenfriedersdorf, good timber has been observed 100 to 200 years old, and even in the "Alten Mann" old workings were constantly wet 300 years old. On the average, under ordinary circumstances of pressure, moisture and ventilation, an age of twenty-five years for oak, and from five, six, to eight years for pine, appears common.

The various modes of preserving timber in a mine, or increasing its durability, may be subdivided into natural and artificial means:

1. As the first of these must be considered that of introducing none but sound wood into a pit, and the immediate removal of all rotting timber, which would infect the sound timber; the use only of barked wood, and where pine is used to avoid cutting it, by using where possible the round wood.

2. A good current of fresh air. This, however, is a matter on which the preservation of the timber must always be but of subordinate influence.

3. A uniform state of either wetness or dryness, the former being preferable. In shafts it will be comparatively easy to comply with this first condition of constant wetness. In most of the shafts in the Hartz this wetting of the timber has received considerable attention, and is generally accomplished by placing flat boards, called "Traufbretter," lengthways in an inclined position beneath the horizontal stemples, in such a manner that they catch the water as it drops from one stemple and guide it on to the next below. These dripping boards, as we may call them, will generally be necessary only between those stemples on the hanging and lying wall of the shaft, underneath the former to prevent the water falling down the shaft or on to the lying wall, and beneath the latter to prevent the water dropping on to the lying wall and running down it, missing entirely all the stemples below. These dripping boards are not necessary at the ends of the shafts if they are, as in general, in a vertical plane, since the stemples are usually fixed in inclines alternately toward one side of the shaft and then the other. The wetting of the level timbering can usually only take place when the water has been collected in the shaft and carried in pipes along the levels. At suitable places the piping, generally of wood, has a small bunghole cut in it, in which a hollow conical stopper can be inserted; the mouth of the stopper is closed by a perforated zinc or tin plate, out of which the water under a pressure in the pipes squirts, as from the spout of a garden watering can, on to the timbering in the levels. The wetting of the timbering for its preservation is also practiced at Joachimsthal, and at Neurode, in Silesia. If the timber cannot be kept regularly wet, it is better to let it remain constantly dry than alternately wet and dry.

The object of the artificial means of preserving timber is to remove the nitrogenous compounds, which are chiefly contained in the sap and which are the first cause of the rotting of the timber, or to preserve them from contact with air, which would induce an oxidation of the nitrogenous compounds. The methods which have been employed may be divided into three classes:

1. The coating, or covering over, of the outside of the wood with some substance which will preserve the sap from contact with the air. One of the simplest examples of this mode of preserving the timber from the action of air (though not coming directly under the category of coating) is the charring of the timber, which thus covers the outside for a short depth with a layer of charcoal. The substances more or less generally used for coating timber are coal tar, lime, cement, vitriol, etc. The timber should be coated at the surface before being brought into the mine and built into its place. Barked timber only is used, and should be coated, not only on the outer barked surface, but on all cut surfaces, so as to prevent as far as possible any dampness getting into the timber. In many cases results are said to have shown that the timber lasts twice as long, and in others that, on the contrary, the timber has rotted sooner. In all probability in the latter case the wood itself may have been unsound to commence with.

2. The removal of the sap either by means of water or by steam. When water is used this is best accomplished by forcing the water through the tree, washing or carrying off the sap with it. This is generally accomplished by placing the trunk of timber to be treated with the root end upward (after the root and thin end have been sawn square off). The root end is inserted in a cylinder about 6 ft. high, the connection between the tree and cylinder being made by means of an India-rubber belt, or other material by which the junction can be made water-tight. The water is forced into the cylinder at a suitable pressure, and entering the pores of the wood passes along them, and washing out the sap flows out of the top (small) end of the tree. The operation is continued until the water is no longer colored or thick, but perfectly clear, requiring for a length of 10 feet of pine about eight hours. Sometimes steam is used instead of water, the apparatus being essentially the same. After the completion of this operation the wood is carefully dried. While steam has the advantage that the air is thereby removed from the pores of the wood, they have both the disadvantage that on drying the pores of the wood are left exposed to the air, which renders the wood liable to destruction from the oxidation of the fibers. When the empty pores of the wood are immediately filled with a solution of some substance we have—

3. The mode of artificially preserving the wood by impregnation. The substances for impregnating timber are introduced in solution, generally under a greater or less mechanical pressure. The following summary we take from Bergrath Lottner's "Manual of Mining:"

The saturation by simple immersion in a solution of common salt is often practiced in the brown coal mines of Saxony. At Zscherben wood which had been so saturated was found after 13 years perfectly sound in places where wood which had no artificial treatment would have become so rotten as to require replacing in two years, and similar experience is said to have been obtained at Tollwitz, Nietleben, Eisdorf, Eggersdorf, Altenweddingen, and Stassfurth; and it is, moreover, a well-known fact that in salt mines wood is but little liable to rotting, although it is found that wood saturated with such a solution becomes brittle.

A solution of sulphide of barium, proposed by Ruttger, and tried by the Eschweiler Mines Institute, is said to have given good results. A solution of chloride of zinc has been tried on sleepers for an inclined plane at the surface of the Ibbenburen Colliery, and also on sleepers for the Hanoverian railways, and is said in both cases to have given excellent results; on the contrary, however, experimental trials with this method in the Hartz mines are said to have given unfavorable results.

At Chemnitz two methods of impregnating timber with a solution of sulphate of zinc and copper were tried, the one proposed by Lüdersdorf, containing 4 per cent. of sulphate of zinc and 1·6 per cent. of sulphate of copper by simple immersion, and the other proposed by Boucherie, containing 3 per cent. of sulphate of zinc and 2 per cent. of sulphate of copper introduced under pressure. The results of the trial of the first method showed that by the simple immersion the pine wood had not been thoroughly penetrated, for at the end of three years, although the core was rotten, the outside of the wood was quite sound; oak seemed to have been thoroughly penetrated by the simple immersion, since it was found quite sound; pine wood in the same mine which had had no such treatment was found completely rotten, and oak had been attacked to a depth of ¾ in. Both kinds of wood which had been treated by Boucherie's method were found quite sound at the end of three years. Lottner recommends that the wood which had been prepared by these two methods should have been treated a second time in a solution of chloride of lime or sulphate of barium, by which sulphate of lime or baryta would have been formed. Timber which had been treated in a solution of sulphate of lime (gypsum) was found attacked at the core, but sound on the outside, at the end of three years.

LIQUID FUELS.

RECENTLY at the Institution of Civil Engineers, London, the paper read was on "Liquid Fuels," by Mr. H. Aydon. It was stated that apparatus specifically adapted for the combustion of liquid fuels, which comprised every class of fluid hydrocarbons, might be ranged in five classes. The leading principle of their action was either the subdivision of the liquid as spray, or by percolation through a porous bed, or by preliminary conversion into vapor—when the fuel was mixed with air, or with air and steam, by the instrumentality of jets of steam or of compressed air—or it was burned simply as gas in jets. The earlier system of Mr. C. J. Richardson, in which the liquid fuel, mixed with heated air, percolated upward through a porous bed, was tried at Woolwich Dockyard, but the performance was not satisfactory, for black smoke and soot were discharged in such abundance as speedily to choke the flue-tubes and stifle the draught. By a subsequent improvement, in which a mixture of steam was introduced with the fuel, a much better performance was effected—the quantity of water evaporated having been increased from 6½ lbs. per pound of fuel to from 7 lbs. to 18½ lbs. per pound of fuel, though the formation of dense smoke was not prevented. The performance under the same boiler amounted to an evaporation of 8 lbs. of water per pound of coal.

The system of Messrs. Simm & Barff, in which the liquid fuel was vaporized in a retort placed in the furnace, and burned in jets, was tried in 1866 on board the yacht Minnie. The quantity of coal consumed amounted to one-third only of the corresponding quantity of coal. The system was afterward tried with the addition of steam, and with better results, as the intensity of the combustion was increased, and smoke was prevented.

In the fourth system, patented by the author in conjunction with Mr. Wise and Mr. Field, in 1865, the liquid fuel was summarily vaporized, by the injection of the liquid into the furnace by the instrumentality of steam, which might be superheated, the supply of air for combustion being at the same time drawn in as an induced current. By this plan the materials could be instantly and thoroughly mixed and converted into vapor or gas before ignition took place. No alteration of the ordinary furnace or grate was needed, so that either coal or oil could be used. For burning oil the grate bars were covered with thin fire slabs and a few cinders, and the ash-pit doors were closed to keep out surplus air. In March, 1867, this method of burning liquid fuel was tried at the works of Messrs. J. C. & J. Field, South Lambeth, in a Cornish boiler of 20 or 22 horse power, 5 ft. 6 ins. in diameter, with a 3-ft. flue. The results of several days' experiments showed an average of 19½ lbs. of water evaporated per pound of liquid fuel. The boiler previously evaporated 6½ lbs. of water per pound of Aberdare coal. Similarly, experiments with a double-flue Galloway boiler at the chemical works of Mr. Barnes, at Hackney Wick, gave a net evaporative performance of 25·3 lbs. of water per pound of fuel. Experiments had been made with other boilers, in which the evaporative efficiency of the liquid fuel ranged from 1½ to 3 times that of coal. Equally good results in favor of liquid fuel were obtained from its employment under a marine boiler at Woolwich Dockyard.

The fifth system enumerated, the invention of Mr. Dorsett, in which the liquid fuel was vaporized in a separate boiler or retort, to be burned as a gas, was tried in 1868 at the chemical works of the inventor at Deptford, and also on board the

Retriever steamer. The results in favor of liquid fuel showed a reduced consumption in the ratio of 2·5 or 2·7 to 1 as compared with coal; but against this economy had to be placed the cost of the separate generators and their furnaces, and of a force-pump. The retort, too, was liable to explosion, in consequence of the deposit of solid carbon within it.

The first employment of the author's system in metallurgical operations was at the Millwall Ironworks. The oil furnace was an adaptation of an old scrap-iron furnace 7 ft. square and 2 ft. 9 ins. high, having a fire-grate 2 ft. 6 ins. wide and 7 ft. long. It was fitted with bricks, and was used as a combustion chamber. Three oil injectors played directly upon the metal to be heated, lengthwise of the furnace. In August, 1871, the results of comparative experiments showed that—

The time taken to heat the furnace with coal was six hours.
" " by above system (five) was 5 h.
" " by the author's system (four) was 2 h. 6. m.

In five hours and a half—the total time occupied in making up, together with the intervals between the charges—68¼ gallons of liquid fuel were consumed, being at the rate of 12½ gallons per hour; 21 piles of scraps, each of 2 cwts., were charged into the furnace, or a total of 42 cwts., while the weight of iron taken out was 37 cwts. 20 lbs., showing a loss of 4 cwts. 92 lbs., or 11⅛ per cent. The furnace consumed 6,720 lbs. of coal in 12 hours to keep up the heat against 1,405 lbs. of liquid fuel, showing a ratio of 4¾ to 1 in favor of the oil, the consumption of which was at the rate of 17 lbs. per hundred weight of iron. The loss of iron treated by coal was from 22 to 25 per cent., whereas with liquid fuel it was only 11⅛ per cent. by system four, and 16⅛ per cent. by system five.

The author in 1875 proceeded to Canada to experiment on the reduction and smelting of the refractory iron ores of that country, many of which were magnetic, and contained 32 per cent of titanic sand. The sole object of these trials was to prove that the Canadian ores could be successfully treated by means of petroleum as fuel. The experiments proceeded satisfactorily, and pig-iron was readily produced from the ore, though it was found that oil fuel in smelting operations must be used in connection with an air-blast under pressure. The experiments were interrupted by the severity of the weather, and not until recently had the refractory and other ores been smelted and reduced at a minimum cost by liquid fuel or native petroleum.

The results of the use of liquid fuel in Russia and other foreign countries were given in the paper, together with the conclusions of Mr. Isherwood, Chief Engineer of the United States Navy, on its employment in steamers. The author made the general deduction that although liquid fuel might be burned without the employment of steam, yet it was consumed most economically and with the best results in the presence of steam; and, of course, the more highly superheated the steam the better was the performance.

AUSTRO-HUNGARIAN HIGH GRINDING.

PROFESSOR FREDERICK KICK lately delivered a lecture before the German Polytechnic Institute, Prague, on the system of milling at the Buda-Pesth mills. He said that he did not intend to give a dry account of an excursion to the Buda-Pesth mills, stating what he saw there, but rather to give the gist of the thoughts things he had seen had given rise to.

The substance of his remarks were as follows: The first thing that strikes the observer is that the Buda-Pesth mills have in the last few years undergone altogether astonishing alterations and improvements, the most important of which is decidedly the introduction of roller mills. Although perhaps all mills are not put on so perfectly modern a basis as the Fabrikshof (Ofen-Pesth Steam Mill), yet in all large mills the stones are replaced by rollers for all stages of grinding except the ending and grinding of bran, and new middlings purifiers, particularly the Carl Haggenmacher machines, and also centrifugal silk-dressing machines (the latter not universally), have been put up. The introduction of roller mills into the large mills of Pesth is in itself sufficient proof of their economic value. Granted that those large milling establishments, which in many cases each grind considerably more than 1,500 cwts. of wheat per day, have greater liberty of action in choosing and adopting new machinery, by reason of larger capital, than small mills, which do not work continually, but only do job lots, still there can be no doubt that even for smaller establishments roller mills are decidedly advantageous. In Pesth it is calculated that 10 to 15 per cent. more good white flour is made by adopting roller mills, which gives an additional value of 40 kreutzers for each cwt. of wheat. If this statement of Adolph Fischer is compared with that of Otto Müller, the latter being to the effect that with roller mills it is calculated that each indicated horse-power will grind 2·7 to 5·5 cwts. of wheat, as compared with 3·3 cwts. ground with the same power by millstones (thus showing in one case an increased and in the other a smaller absorption of power, or of its equivalent fuel), we find this grain altered in the first case to 38 kreutzers and in the latter to 43 kreutzers. This great variation in the quantity of wheat ground by each indicated horse-power is well worthy of notice, and it will therefore be further mentioned.

First of all he considered the different rolls themselves, regardless of the construction of the roller mills. Commonly speaking, by rollers are meant those with smooth faces. The action of these is, indeed, a squeezing one, even then, if both the rolls have not the same speed. But the action differs totally if grooved rollers are used, such as are, for instance, employed by Ganz & Co. After comparing these chilled-iron grooved rolls with the old grooved steel rolls, Professor Kick noticed the Zipser wheat cutting machine, the favorable results obtained by the same being attributable to a slight shearing action; but in his opinion chilled-iron rolls are more durable than these, the face of which consists of toothed steel rings. As regards grooved rolls, stress was laid on the fact that they are used with better advantage in the large Pesth mills than in small mills, because in the former for each break, and for the reducing of the coarse broad bran, rollers may be made use of, specially grooved in point of breadth or fineness of grooves, whereas the same rolls will have to do a variety of work in small mills. After a few remarks respecting porcelain rolls, which are mentioned as not to be recommended, the speaker proceeded to the question, "How is it that roller mills can require so varying a motive force for grinding the same quantity?"

The reason he finds to be in the variable friction in the bearings, which differs according to the size of the bearings and the pressure. In small roller mills, as, for instance, the Wegmann-Ganz, the Henry Haggenmacher, the small Escher Wyss & Co., etc., roller mills, the loss of power thus occasioned amounts to 1·2 to 2·3 horse-power for two pair of rollers, taking the co-efficient of friction only at 0·06. In large roller mills for grinding middlings this loss of work increases to as much as 5 horse-power for one pair of rollers. The result is that the maker of the roller mill has to take this into account in choosing his dimensions and the pressure. The miller ought also to make due use of the screw regulating the minimum distance of the rollers from each other, and to work only with the absolutely necessary amount of pressure. A further consequence of the above is that the owner of small mills should avoid the roller for grinding middlings, as absorbing too much power, and rather to use the stones as heretofore for grinding the pure or clean middlings, because the roller mills for middlings necessarily require a great pressure. The principal advantage of the roller mills consists without doubt in the making of a larger amount of a good semolina and clean middlings, but whether it is profitable to grind the pure fine middlings into flour is doubtful, particularly as long as the efforts of reducing the friction have not been more successful than heretofore. As regards the various constructions of roller mills, Professor Kick remarks that generally they do not influence the quality of the flour. The old roller mills designed by Salzenberger and made by Escher Wyss & Co., having three pair of rollers, one above the other, made as good a quality of flour as modern roller mills. A proof of this assertion is the "Walzenmuehle," in Pesth, and the assertion cannot be contradicted by saying that the old roller mills were unable to make their way into universal use, because the reason why the former were not more widely adopted was their extremely high price.

Against the use of the so-called reducing roller, which, being itself grooved, works against a grooved segment and loosens the meal, no perfectly justified objection can be made; for, although it is true that the bran may be sometimes ground up, still, as in that case roller and segment were made to work against each other too much, it is more the fault of the workman than of the machine. The detacheur loosens the meal coming from the roller excellently well, and its work cannot be made bad by carelessness, as in the case mentioned; but this is almost its sole advantage, and it is not absolutely necessary in small mills. It is evident that, instead of a detacheur, a disintegrator may be used for loosening the meal, but it is also clear that the more vehement the action the more the quality of the flour will be deteriorated. Professor Kick referred to the difference in the shade of flour made from the same pure middlings by rollers and by stones, and stated that it was a fact that flour made by the latter was of a yellower tint than that made by rollers from the same purified middlings. Rollers with differential speed give a more yellowish flour than those rollers for grinding middlings into flour which run with equal speed. Chilled-iron rollers give a more yellowish flour than porcelain rollers, and millstones which are not ventilated a more yellowish flour than those with ventilation. What can be the reason of these differences? He had heated fine yellowish wheaten flour No. 00. The experiment was made so that the flour was suspended on saucers in a hot-air bath, and the temperature carefully raised to 100° C. (212° F.), when, on being examined, not the slightest difference in the color was visible, not even by the application of Pekar's flour test. Raising the temperature to 158° C. (316° F.), the flour became a little less yellow and rather darker, but the difference in the shade of flour hardly amounted to one number. It was only after heating the flour to 262° C. (392° F.) that the well-known brown color appeared. From these experiments it is apparent that the difference in color cannot be caused by heating during the process of grinding. The same wheaten flour, No. 00, rubbed in an agate mortar in very small portions, and at a pretty great pressure, showed a decisive whiter color. Of two flours, therefore, all other circumstances being equal, the one more finely reduced will have the whiter color. How can the result of this experiment be brought into accordance with the above facts?

If the runner of a pair of millstones at work is taken up, all furrows of the bedstone will be found to be full of fine flour; the disintegration is therefore carried on on a bed of flour, and it is consequently less intense; the result is that the flour, because coarser, is more yellow. If, however, the flour is blown out by ventilation, the action of the stones is more intense, the bolster of the flour is partly removed, and the flour is more finely ground, and, therefore, whiter in color. Flour from rollers can only be made whiter than that by the stones, from the same pure middlings, through the former grinding finer than the stones, and through being better freed from the fluffy particles of middlings. It is well conceivable, and in conformity with the above statement, that the rough porcelain rollers give a whiter flour under the same circumstances than the smooth chilled-iron rollers. It seems a contradiction that rollers with differential speeds should give more yellowish flour than those with equal speeds; but this apparent contradiction is removed if we take into consideration that at present the rollers having equal speed are all worked with greater pressure than those having differential speeds. As regards centrifugal dressing machines, Professor Kick remarked that he has seen them in various Pesth mills, and that they were particularly esteemed for dressing soft middlings. Their principal advantage he considered to be the smaller space required. Referring to new semolina purifiers, those of C. Haggenmacher were specially mentioned as excellent, and as being used in many of the Pesth mills. Finally Pekar's flour test was noticed, and illustrated by an experiment.—*Oesterr. Ung. Mueller Zeitung.*

PAPER NEGATIVES.

By CAPT. W. DE W. ABNEY, R.E., F.R.S.

I HAVE for the last few months been engaged in some work in which it seemed desirable to employ paper instead of glass as a basis for the ordinary emulsion; and, after various trials, I found that the following plan answered well in every respect, more particularly as regards cleanness of the back surface of the paper, while with careful manipulation the negatives develop admirably. Ordinary smooth Saxe paper is coated with paraffine made as follows:

Paraffine	50 grs. (about).
Benzole	½ ounce.

To which is added enough India-rubber solution made as under to bring the total quantity up to one ounce:

India-rubber	5 grains.
Benzole	1 ounce.

To make this India-rubber solution, I have found that if bits of the black India-rubber tubing be cut up into shreds and placed in the benzole, in time it will dissolve *per se*, though sometimes it takes months to do so. If there be no hurry, this is the best method of obtaining a solution which is free from all turbidity. The India-rubber just swells up to an enormous size, and finally passes into solution with the exception of a small portion of apparently insoluble matter, which floats in the fluid as a gelatinous mass. This can easily be filtered out. Failing this supply of India-rubber solution, it may be made by dissolving the well-known India-rubber paste in benzole. This is never so free from specks as the last solution, but still it answers the purpose.

To prepare the paper I use a cotton-wool brush, the cotton having been previously well boiled in carbonate of soda, and then thoroughly washed and dried. Through a glass tube of about a quarter-inch internal bore and about four inches in length a loop of string is passed, and a tuft of cotton pulled partially into the bore, a good brush remaining outside. The paper is then brushed over, back and front, with this solution, and the benzole allowed to evaporate spontaneously. A coating of the India-rubber solution (without any paraffine) is next given to one surface of the paper, and, when dry, it is ready for coating with emulsion. The paper is turned up to form a tray, about one-eighth inch side being sufficient, and a small spout is left at the most convenient corner. The paper dish is then supported on a sheet of glass, and the emulsion poured on in the manner adopted for coating a plate; when dry it is ready for exposure.

I need not here enter into the various methods which may be adopted for placing the sensitive sheet in the proper position in the camera or the method of developing, the one being well known to calotype workers, while in the other there is no deviation from the formula usually employed. After fixing and washing, it is advisable to pass a little dilute acetic acid over the image, as this removes any tendency to a pink coloration which is sometimes observable. The image appears (usually) of an olive green tint by reflected light, while by transmitted light it is of a warm black tone. If the last coating of India-rubber be efficient, there is very little difficulty in getting excellent results. If the developer can penetrate to the paraffine there is a danger of a saponaceous matter being formed, which tends, by its contact with the colored alkaline developer, to tinge the whites. This points to the fact that the pyrogallic acid solution of alkaline developer should be freshly made up, and if this be attended to the discoloration in any case will be but slight. I have produced some hundreds of negative pictures on this material, and they can be printed from without being waxed, when the subjects are not of a very small size, or when they do not possess any great minutiæ in detail.

I have not tried enlargements, but it seems to me there is no doubt that they would be effective.—*Photo. News.*

A SIMPLE PHOTO-PRINTING PROCESS.

By M. GOBERT.*

THE communication that I have the honor to make is interesting to the photographer and to the lithographer. From the photographic point of view the process which I am going to describe does not constitute a novelty, since the material employed is nothing but bichromated albumen, whose photo-chemical reactions are perfectly well known. As far as lithography is concerned, the interest appears to me to have a certain importance, since it calls into play a new feature which gives an answer, at least partially, to a weighty question which cannot but occupy the attention of lithographers and printers.

It is known that the quarries which furnish the lithographic stones are not numerous, that they are being worked out, and that France has to depend upon her neighbors for stones of the first quality. The state of things is not new. It is considered grave and important by competent persons, so much so that researches and experiments have been made to remedy it by endeavoring to replace the lithographic stones by other kinds of material. I certainly have not myself undertaken the task of solving this problem, but chance photographic manipulations have brought out some very unexpected and curious results.

Common glass and *plate glass*, directly and without the interposition of any substance, can be used in lithography to replace the stone. It is sufficient merely to roughen the surface of the two kinds of glass, which can be best done by means of acid. It will have been remarked that ground glass is easily and regularly wetted by water, which it is almost impossible to accomplish on a polished plate. It is this property, which up till now has not been utilized, that allows drawings to be made in printers' ink upon roughened plate or common glass. The strong plates of our printing frames are perfect for this purpose.

The following is a brief description of the process:—Bichromated albumen is used, three grammes of bichromate of ammonia being added to one hundred grammes of albumen. This solution, by preference, should be filtered. Covering the plates is very easily accomplished by the very simple means which have so often been described, which goes by the name of the "tournette," or whirling apparatus. The film should be very thin. The plates, which are prepared in daylight, are placed to dry in a dark place. I recommend that they should not be prepared in advance, for they rapidly lose their good qualities. The bichromated and dried albumen becomes insoluble in 6 to 8 hours even in absolute darkness. The sensitiveness of this preparation is very great, an exposure of only a minute to direct sunlight, or four or five in the shade, being sufficient. The light produces insolubility in the albumen, and also, which is more important, a complete imperviousness to water.

Immediately after exposure, a printer's roller charged with greasy ink is passed over the plate in such a way as to give a black coating to the surface, after which the plate is immersed in a dish of plain water. Those parts not acted upon by light almost immediately dissolve, and carry with them the excess of ink; a clean, vigorous, and sharp image is developed, which adheres perfectly to the plate, and is fit for pulling lithographs from in the usual manner, taking care, however, not to use acid gum, since the least trace of acid destroys the adhesion of the albumen. The gum solutions should be absolutely neutral.

The above description shows that the process is only applicable for the reproduction of subjects executed in line. Up to the present time I have not been able to obtain satisfactory or regular photographs in shades.

In conclusion, I would add that bitumen of Judea, applied to a ground-glass plate, should also give images of great purity, and capable of being used for pulling-off lithographs; also, by drawing directly upon ground glass with solutions of gum lac, resin, bitumen, India-rubber, etc., impressions may be pulled from original designs.

**Bulletin de la Société Française de Photographie.*

CONSTITUTION OF THE SUN AS REVEALED BY PHOTOGRAPHY.

By M. JANSSEN.*

FROM the moment in which we entered upon the study of celestial photography, on the occasion of the transit of Venus, we have always thought and maintained that photography appeared to us called to play a leading part in astronomical observations.

This opinion has just received a first confirmation in so far as solar photography is concerned. We have succeeded in obtaining, at the Observatory of Meudon, prints of the sun disclosing important facts on this star which have hitherto escaped optical observations. I cannot here enter into details; I will only say that, following certain theoretical considerations, and the study of the results hitherto obtained, I have been led to combine enlargement of the diameters of the images with diminution of the time of the action of the light in order to combat the photographic irradiation.

The solar images have been successively brought to a diameter of 12, 15, 20, and 30 centimeters. At the same time the duration of the exposure has been reduced in the last experiments to about $\frac{1}{1000}$ of a second, this time having reference to the direct action of the sun without the intervention of a refracting medium.

Under these conditions, and taking the most minute precautions to get images of the greatest possible purity, solar images have been obtained having nothing in common with previous results.

While the older images, apart from spots and faculæ, showed no real detail of the sun's surface, the new photographs show this surface as entirely formed by a fine granulation, the elements of which are very varied in form, in size, in relative position, in clearness, according to the regions, etc. This image, each point of which has been formed in $\frac{1}{1000}$ of a second, would require several months to be studied in all its details.

The size of the granular elements is very varied: grains are to be found from some tenths of a second to three and four seconds in diameter.

This granulation shows itself everywhere, and does not seem, as far as we have gone, to present a different constitution toward the sun's poles. We shall have, however, to return to this point.

But the most remarkable result, and one which is due to the aid of photography, is the discovery of the photo-spherical network.

In fact, the careful examination of these photographs shows that the photosphere has not a uniform constitution in all its parts, but that it is divided into a series of figures more or less distant from one another, and presenting a peculiar constitution.

These figures have generally rounded contours, on approximately rectilinear and rambling polygons. The dimensions of these figures are very variable—they have sometimes a diameter of a minute or more. While in the intervals between the figures of which we are speaking the grains are clear, well defined, although of very variable size, in the interior the grains look as if half effaced, drawn out, strained; most frequently, indeed, they have disappeared, to give place to trains of matter which replace the granulation. This would seem to show that in these places the matter of the photosphere is subjected to violent movements which have disturbed the granular elements.

I will not touch at present on the consequences of this fact, which throws light on the forms of solar activity, and shows, as I said some time since, that this activity in the photosphere is always very great, although no spot may show itself on its surface.

The network of the photosphere could not be discovered by the optical means which are used in ocular observation of the sun. In fact, to determine its existence on the prints, lenses must be employed which enable us to cover a certain extent of the photographic image. Then, if the enlargement be sufficient, if the print be quite pure, and especially if it has received the proper pose, we see that the granulation has not everywhere the same clearness; that the parts with well-formed grains are delineated as currents which circulate in such a manner as to circumscribe spaces in which the phenomena present the appearance which we have described. Thus, to determine this fact, it is necessary, as we said, to cover a considerable portion of the solar disk, a thing impossible to realize when we view the sun with a very powerful instrument, the field of which is very limited from the fact of its power. Under these conditions we can, indeed, determine that portions exist in which the granulation ceases to be clear or even visible; but we cannot suspect that this fact is connected with a general system.

Already the examination of photographs during a few months discloses differences in the constitution of the photospherical network—differences which will give us information on the variations in the forms of solar activity.

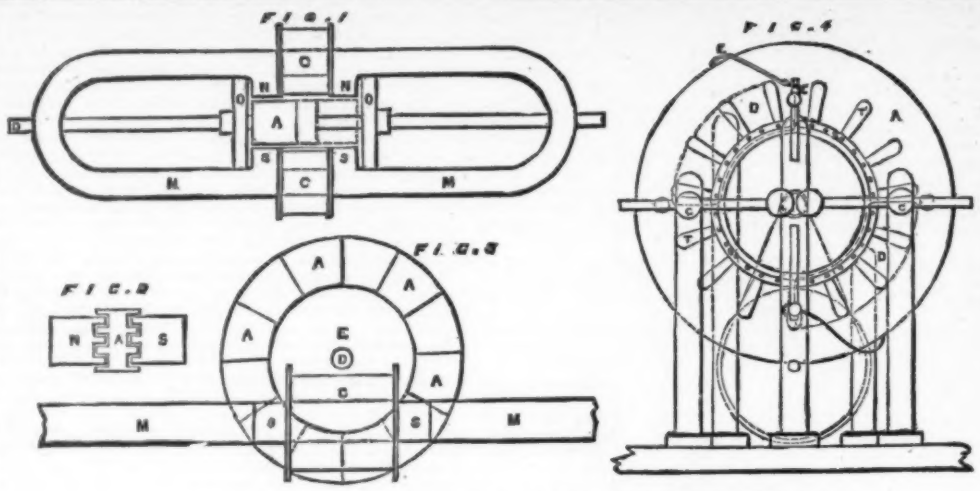
I add this very important fact, established with certainty by the photographs, that of the existence of very dark nebulous tracks, showing themselves in the regions of regular granulation, and which indicate that the photospherical layer must have a very slight thickness.

Solar photography is thus placed, from this time, under conditions in which it can reveal to us most important facts on the constitution of the sun. It is a new method which is opening before us, and the efforts of which we can join to those of spectrum analysis and the older optical methods to solve the great problems which the nature of the sun raises, problems which touch in so many points those of the constitution of the universe.

IMPROVED CARBON PROCESS.

HERR HONIKEL, of Leipzig, has, according to the *Wochenblatt*, proposed the following amendment of the carbon process:—Starting from the assumption that the shadows and half tones of all carbon prints are uncommonly rich in details, while the lights and high lights are extremely poor, Herr Honikel proposes that instead of mixing black or dark coloring matter, as has hitherto been the custom, with the gelatine destined to form the pigment tissue, white pigments should be used, and the sensitive tissue exposed under a positive and developed, and transferred to black or colored paper. Besides the more beautiful appearance claimed for such pictures, this method is said to possess the advantage of indefinitely increasing sensibility. According to Herr Honikel the sensitiveness of white pigment tissue is from twenty to twenty-five times as great as that of ordinary carbon tissue. And yet another advantage claimed is that by means of white pigment tissue it is possible to produce direct enlargements upon paper with the camera. It is scarcely necessary to remind our older readers that the late Mr. William Blair devoted much attention to this kind of pigment printing, an account of it having been published by him nearly ten years ago.

* Read before the French Photographic Society.



VARLEY'S NEW ELECTRO-MAGNETIC MACHINE.

VARNISH FOR MELAINOTYPES.

THE *Photographisches Wochenblatt* gives the following directions for the black backing of ferrotypes or melainotypes:—Cut the iron plate to the proper size and heat it to about 80° R. Then brush upon both sides of the plate the following varnish:

Alcohol	100 grains.
Yellow gum lac	10 "
Sandarac	5 "

The mixture should be slightly warmed for half an hour in a glass vessel, and the clear part decanted off. The black varnish, with which one side of the varnished plate is to be coated, consists of—

Chloroform	80 grains.
Asphalt	80 "
Canada balsam	20 "

When in the course of a few days the varnish has become perfectly dry the plates may be used, but it is as well to dip each of the four edges into the above solution, a sufficient quantity of which for the purpose has been poured into a groove made in a piece of wood. The object of this is to prevent the varnish from slipping off the edges of the plate and thus permitting the iron to come into contact with the silver bath and spoiling it. Before collodionizing rub the plate with a tuft of cotton wool to remove all dust. The most suitable collodion is lithium collodion, which contains a little free acid. The developer is—

Water	500 grains.
Glacial acetic acid	30 "
Alcohol	30 "
Sulphuric acid	30 "

Intensify and fix as usual.

VARLEY'S NEW ELECTRO-MAGNETIC MACHINE.

These improvements have been designed by MR. C. F. VARLEY, of London, Eng.

IN magneto-electric machines constructed according to the invention, actual or nearly actual contact is maintained between the armatures and poles of the inducing magnets. The magnets themselves, together with the intermediate cores surrounded by helices, form a complete ring, link, or circuit of iron or iron and steel or other magnetic substances. These permanent or electro-inducing magnets have their respective north and south poles continuously or nearly continuously closed, notwithstanding the movement of the armature or armatures, but the armature or armatures (when rotated or moved to and fro along the iron or link) effect the commutation, or, in other words, alter the direction of the magnetic conduction through the inducing cores surrounded by electric conductors, the effect of which commutation or reversal of magnetism is to generate electricity in the conductors forming the helices. In arranging a machine on these principles in the simplest and most elementary form, two horseshoe magnets are placed opposite to one another, and between their poles are two soft iron cores on which coils of covered wire are wound. The two north poles of the magnets are in contact with the two ends of one of the cores, and the two south poles are similarly placed in relation to the other core. Together with these, which are fixed parts of the apparatus, an armature is employed, to which a reciprocating motion is communicated, which places it first in contact or nearly so with the two poles of one magnet, and then transfers it to a corresponding position in respect to the other magnet. The faces of the magnets and of the armature may advantageously be grooved to increase the area of the surfaces in contact or close proximity. The armature may also advantageously be made double, so that the poles of the magnets may be received between its two parts. The attraction of the magnet for the two parts of the armature will then balance each other, and by a screw adjustment the surfaces of the armature may be brought very close to the surfaces of the magnets without giving rise to friction. The dimensions of the armature should be such that it may close with one pair of poles before leaving the other pair of poles. In place of a reciprocating armature a rotating armature may be employed, so formed as to connect the north pole of one magnet with the south pole of the other, and as it rotates to couple the poles alternately.

By another arrangement, six or it might be any other even number of horseshoe magnets are arranged radially around a circle, and with, say, the north poles of all the magnets uppermost. As in the former case, the similar poles of the magnets are connected by means of soft iron cores wound with covered wire forming the circuit in which the currents are generated. These, which are the stationary parts of the apparatus, are all preferably carried upon a gun metal cylinder or ring, within which a corresponding plug or frame rotates; this plug or frame carries armatures, and as the plug revolves these couple together the poles of each magnet in succession. In some cases, to produce currents for telegraphic and other uses, Mr. Varley gives motion to the armature by a finger-key when it is desired to transmit a current. In arrangements of this nature the armature is of small size and the magnets have pole pieces or horns approaching within a short distance the one of the other, leaving only sufficient space for the armature to move between them. For some purposes he mounts the armature on a tuning-fork, and so obtains electric pulsations in unison with the fork. Other vibrators may be substituted for the fork. In Fig. 1, M, are horseshoe magnets—permanent magnets are represented, but electro-magnets are also available. Between the magnets and in contact with them are two cores of soft iron wound with coils of insulated wire, C; they form part of the circuit or circuits in which the electricity generated by the apparatus is transmitted. N, S, mark the poles of the magnets. O are guides made of gun metal in which the rod, D, is free to slide to and fro; it has upon it the armature of soft iron marked A. A reciprocating movement may be communicated to the armature between the guides, O, by connecting the rod, D, directly or indirectly with a steam engine or other motor. It will be observed that in this arrangement no reversal of the magnetism in the armature, A, takes place. Fig. 2 is a section showing the armature grooved to increase the area of the surfaces in close proximity, when so desired; but in some cases Mr. Varley forms the armature in two parts—one above and the other below the poles of the magnets, with a gun metal block between them.

Fig. 3 represents an arrangement in which a number of armatures are mounted upon a rotating disk of gun metal, and they are so placed that as the disk rotates they close the poles of the two magnets alternately. The armature is mounted upon a rotating axis, D, and as it revolves the magnetism in the armature, A, is reversed in each rotation. The armature may carry a coil, in which case electricity will be generated in the circuit of which this coil forms part. An ordinary rotating commutator may be included in the circuit, and the electricity can then be employed to excite the magnets, M, M, when electro-magnets are employed. Mr. Varley describes several other methods of arranging the apparatus for special purposes, which, however, it is not necessary to reproduce.

The electrostatic multiplier referred to as the second part of the invention comprises a series of insulated studs or conductors which act as potential carriers, and enable the tension, which on first starting may be so minute as to be imperceptible, to be augmented as described in the prior patent referred to, but in addition to which he now employs surfaces or disks of a dielectric substance, such as a plate or cylinder of glass or vulcanized caoutchouc, etc., together with inductive surfaces placed on the reverse side to the collecting and discharging combs or contact pieces, by which means a succession of sparks or electrostatic discharges can be maintained. The charging-up or accumulation of the electrostatic potential is effected automatically, and, as well as the former part of the invention, is self-charging. During the operation of charging-up, contact making springs or contact pieces are employed to make contact with the carriers. As soon as a charge of a certain potential has been accumulated these springs are no longer required, and they are removed or thrown out of action.

The electrostatic multiplier, as Mr. Varley prefers to construct it, consists of a vulcanite disk mounted upon an axis carried upon insulating standards, and capable of being rapidly rotated. On the face of the disk strips of tin foil are fixed radially and at equal distances apart. In front of the disk, and diametrically opposite the one to the other, are the two collecting combs by which the electricity passes from the face of the disk to the conductors of the machine. As the disk revolves the strips of tin foil pass in succession in proximity to the collecting combs. Also in close proximity to the disk, but on the other side of it, are two pieces of wood mounted on insulating standards and serving as inductors; they are about the same width or somewhat wider than the collecting combs. They are of curved form, each inductor covering about a quadrant of the operative part of the disk or that which passes near the points of the collecting combs, and upon which part on one side of the disk are the strips of tin foil. The inductors are so fixed upon their insulating supports that the leading end or commencement of the inductor is immediately opposite to the collecting comb, but on the other side of the disk. For the purpose of charging the inductors there are other combs similar to the collecting combs and similarly placed, but removed from them by an angular distance of ninety degrees, so that they are opposite to the ends of the inductors but on the other side of the disk. These combs are placed in metallic connection with the inductors, each to each. To render the machine self-charging, the last mentioned combs also carry springs or wire brushes, which make metallic contact with studs upon the face of the disk and in metallic contact with the tin foil strips upon it, each to each. As soon as the machine is at work these springs or brushes can be lifted out of contact by means of silken threads or other

convenient attachments. Fig. 4 is a front elevation of the electrostatic multiplier: A is the vulcanite disk on an axis carried by insulating standards. The axis has upon it a pulley which receives a driving cord from a wheel rotated by hand; T, T, are tin foil strips fixed upon the face of the disk, and S, S, are studs in metallic connection with the strips, T; C, C, are the rods to which the metallic collecting combs are attached, and D, D, show the position of the inductors at the back. E, E, are the combs for charging the inductors; they also are insulated, and they are connected by the covered wires, E', with the inductors. S, S, are the springs or wire brushes for making contact with the studs. The contact is made when a finger key (not shown) is depressed. The key acts through silken cords which draw the brushes or springs forward; when the key is liberated they move back out of contact. The magneto-electric machines and the electrostatic multiplier are employed in combination for the production of the electric light. Mr. Varley causes an electric arc to be formed between carbon points by the magneto-electric machine, as is well understood, and in order to render the light constant he applies the electrostatic multiplier, by placing its conductors near the carbon holders, so that a stream of sparks may pass to them constantly so long as the light is being maintained. The high tension electricity which in this manner is caused to pass between the carbon points effectually maintains the magneto-electric discharge.—*English Mechanic.*

THE PHONOGRAPH.

Lecture by Professor J. W. S. ARNOLD.

IN response to an invitation from several prominent citizens, Professor J. W. S. Arnold, of the University of the City of New York, lately delivered a lecture at Chickering Hall upon Mr. Edison's recent invention, the talking machine, known as the phonograph. The lecture, says the *N. Y. Tribune*, from which we take our report, was in great part an exposition of the theory of acoustics; the performance of the phonograph was shown to be in strict accordance with that theory. All the experiments shown were quite successful, the Lissajous curves, shown by the electric light, being especially varied and brilliant in their

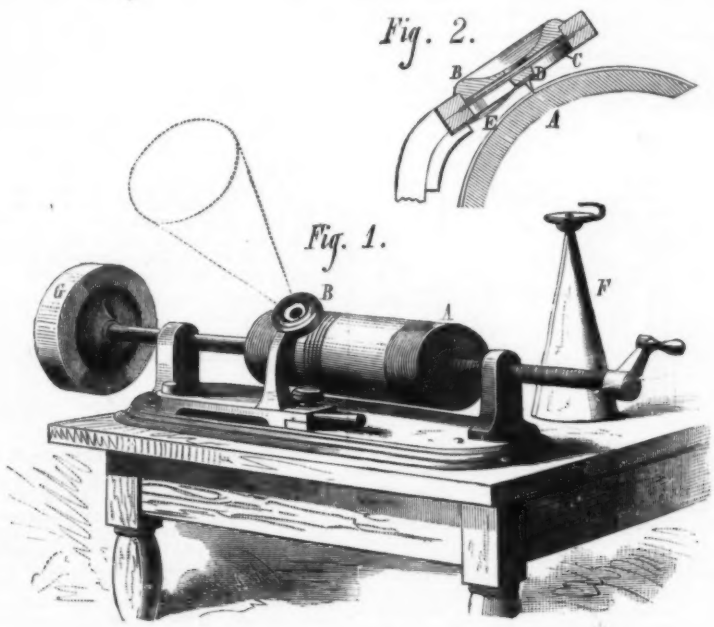


Fig. 2.

Fig. 1.

THE PHONOGRAPH.

changes of combination. Professor Arnold introduced a singing telephone, which afforded much amusement; it reproduced, in the musical sounds of a guitar, the notes of songs sung in another room by his assistant, Dr. Miller. An exceedingly interesting experiment showed on the screen the vibrations which are caused by the voice in enunciating different syllables; the figures thus obtained flashed only for an instant into view, but they were evidently geometrical forms. The phonograph machine repeated the words spoken to it loudly and clearly, so that it was heard in every part of the hall and gallery. The tone is slightly metallic and has a strained effect, very much like the voice of a ventriloquist; it is evidently in need of further improvement in quality and distinctness.

THE LECTURE.

I suppose that most of those who have come on this occasion have gathered themselves here for the simple purpose of hearing the celebrated machine of Mr. Edison talk. After a while we shall see, I think, that the speaking phonograph, or talking machine, is probably the greatest invention in acoustics or the greatest acoustical phenomenon of this century. In order, however, to appreciate the mechanism by means of which the human voice is reproduced in such an extraordinary manner, we must first take a rapid glance at the physics and physiology of hearing.

The physics and physiology of hearing—like that of sight—must be studied in two ways: first of all, the actual conditions which are necessary to produce sound, and then the manner in which sound is appreciated by the individual; in other words, we must become fully acquainted with the objective and subjective phenomena, and then bring these two together. In the first place, both light and sound are phases of motion. Light consists, as we know, of undulations or waves, and sound consists of undulations or waves also; but the undulations or waves of light differ in the manner in which they travel, in which they make their journeyings. The light waves are propagated from a luminous body, and take their direction in straight lines from the luminous body, but the direction or motion of the particles of luminous ether are at right angles to the direction of the propagation. On the other hand, sound waves are transmitted in a direct line, and we have very much the same phenomena in sound as we have in waves of water; that is to say, when a stone is thrown into the water you will notice that from the point where the stone impinges upon the water a series of waves starts, spherical in form, and are sent in all directions outward. In order to appreciate the manner in which these waves pass in the atmosphere (because it is necessary we should have some medium to propagate or transmit the waves) we will make use in a moment of a little mechanical contrivance or diagram, which gives us an idea of the propagation of sound. The waves of sound, as they appear in the atmosphere, consist of rarefications and condensations of the air. Sound can be produced by motion or vibrations in solid bodies as well as in the atmosphere, and we shall see as we study the subject that the ear can appreciate the sensation of sound simply by the vibrations of solid bodies, provided the solid bodies are able to transmit the vibrations directly to the portion of the ear which receives the impressions. But for the general condition of sound we must have air; we must have some substance to carry the waves from one part to the other, and in general, every body, then, that produces a sound on our earth, unless it be the direct transmission of the body, stirs up these waves, and they are carried through the air.

THE MOTION OF SOUND WAVES.

Here we have [illustrating on the screen] what would represent the waves of sound—these light points—and, as I told you, the motion of the particles, which we will study more definitely in a moment, is backward and forward. Now, when a sound is produced, the air is thrown into just the vibrations that we see here. Each one of those waves makes its excursion up and down in all directions. These waves have actually been seen. A physicist of the name of Toepler was enabled, by causing a sound from the electrical discharge, to see the waves of sound, and he found that these waves could be refracted and reflected, and that they obeyed all of the laws which were found in light. In addition to this he found that those waves were spherical, and we have an idea of the propagation of the sound of these waves as spheres overlapping each other, each made up of little particles of air, and these particles vibrating backward and forward. Now we will try an experiment. Here we have a slit-like formation on the screen, and in this we will be enabled to see the vibration of the particles. Each leaves its position only for a short distance. The intensity or the loudness of the sound depends upon the distances each of these particles moves. By the generation of the sound it moves from the position it has at rest to a certain position and back again in an opposite direction, as you see represented on the screen. So far, then, for the generality of the sound waves. There are a great many other points which we have to hurry over to-night in order to get a general and, unfortunately, somewhat superficial idea of the whole subject.

Now, take into consideration the manner in which these waves can be thrown into action. Thunder is produced, as we know, by an electrical discharge in the atmosphere. The electricity being driven through the atmosphere with tremendous rapidity, causes the air to be thrown into vibrations, which cause the sound we recognize as thunder. The electricity is the lightning, the sound is the thunder. If we agitate any body which is solid, we then produce a certain number of vibrations in the atmosphere, and these correspond to the vibrations of the body. If we cause a column of air to vibrate in a tube, we produce a musical tone, which is simply due to vibrations of the air in the confined space of the tube. A tuning-fork in sounding is simply a mechanical disturbance of the air, and, when in vibration, it produces what is known as a musical tone. We shall see that the musical tone depends upon certain peculiar conditions of vibration, and we shall see also that noises differ from musical tones simply as they can be referred to other peculiarities of vibration. In sounding a fork we have a pleasant tone to the ear. This is a mechanical vibration of the air by the vibration of an elastic body, viz., the steel of which the fork is composed. In a moment we will try another experiment where we throw into vibration a column of air confined in a tube. We can also produce vibrations by a succession of puffs. Here is a little instrument called a "siren." It has a disk which is perforated, and an air-chamber. There is now compressed air in the cylinder, and, as it is turned on, the puffs of air escape, causing it to come opposite the opening in this air-chamber very rapidly and evenly, and there is a succession of puffs which can be heard near by, and pretty soon a tone will break forth. [Here the lecturer illustrated his meaning by the apparatus referred to, and was applauded for the success of his experiment.]

SOUND PRODUCED IN TUBES.

The mechanism of the production of sound by this other little instrument is first the generation of hydrogen by decomposing water with the aid of acid. There is a very fine point, from which the hydrogen burns, introduced into a glass tube. The air passing up through the tube becomes heated and is thrown into vibration by the flame, the air being thrown up and down. If we have a very large tube we shall have a very deep tone, and shriller in proportion as the size of the tube diminishes. The vibrations are not so rapid in the large as in the smaller tubes, in the latter case the waves being smaller and consequently lighter in tone. The ordinary effect produced in the organ pipes of course we understand. Here we have simply the puffs of air which pass through tubes of various kinds and we get the various tones, as we shall see. [Illustrating the point by sounding certain stops on the organ.] Here we have the air thrown into vibration simply because the bellows, which is on the inside of the organ, drives the air through the opening into the pipes, and into certain pipes which are known as the diapason pipes, and shocks the air and causes the column to vibrate in the interior of the tube. You see in this tube that I have here, and hold over the flame, the air is thrown into vibrations on account of the heat of the burning hydrogen. [Illustrating it. Applause.] There is another method of producing almost the same general effects which almost every one has heard, and that is the vibration of plates or slips of metal which are met with in the little toy called the metallophone.

Now, then, about the manner of measuring the wave-length of sound. How do we know how long a wave-length is? In measuring it we must have some standard of measurement. First, we must know exactly how rapidly sound travels; secondly, how rapidly the body vibrates. Here is a tuning-fork which vibrates exactly 512 times in one second, and at about the temperature of the atmosphere of this room the sound will travel about 1,120 feet. If I sound this fork we will have wave-lengths which measure exactly two feet two inches, and the manner in which we arrive at that conclusion is by dividing the rate at which sound travels by the number of vibrations in one second. There are instruments, of course, by which we can measure very accurately the number of vibrations any body produces. Among other instruments we have the siren, which I have shown you; and if we produce a tone by this fork, which corresponds exactly in pitch to the tone produced in the siren, we can read off in the index to the siren the number of vibrations produced in a second.

MUSIC COMPARED WITH NOISE.

Now, we come to the question of the difference between noises and musical tones. A noise is the result, of course, of vibrations, but the vibrations are irregular. They do not follow each other at regular intervals, whereas in every musical tone we have the vibrations following each other at perfectly regular rates. The result is that the ear is pleased with a succession of regular intervals of vibration and displeased with those which are irregular. A tone and a noise, then, differ simply in this respect, and, as I have already told you, the pitch or the height of the note depends upon the number of vibrations produced in a given time. If I start the siren once more and allow it to run for a certain length of time, you will notice that as the speed increases the tone becomes higher and higher, until finally we can get quite a shrill tone from what was at first quite a low tone. [The Professor at this point performed the experiment satisfactorily amid applause.] Now, what is the difference between the timbre, or quality, of the note produced by the siren, that produced by the violin, by a canon, and finally by the human voice? What is the mechanism of production? It can be answered in this way: Every time a musical sound is produced, such as we would have in the plucking of a string or by the sounding of a pipe, we hear not only one sound, but a great many sounds, and these other sounds are known as over-tones or harmonics. There is the fundamental tone, which is the principal one, and which we can distinguish by sounding the tuning-fork carefully —— thus. Now, see if I can produce another tone from the tuning-fork. [Doing so.] There is a different one, and here [producing still another tone] is still a different tone. Both of these are sounded when the fundamental tone is produced, but they are not nearly so loud as when we make them especially. These over-tones, then, give the peculiar quality to the various kinds of instruments and to the human voice, and we find these over-tones are produced in the same mathematical ratio. For example, if we take the note "C" as the starting point, we find its first over-tone is its octave, which has twice as many vibrations as the original note. Then the next over-tone or harmonic is the fifth above this octave. Then we have "C," and then "E flat," and so on. We have then a succession of tones which overlap or are mixed with the fundamental tone, and these tones have the ratio of vibration of 1, 2, 3, 4, 5, 6, 7, 8, 9, 10. That is to say, the first over-tone or octave of the fundamental tone has twice as many as the second, the second three times as many, the third four times as many, and so on. Now, it depends upon the number and loudness of these as they mix with the fundamental note whether there is produced either the sound of the violin, or that of the trumpet, or of the flute. If we take, for example, on the organ a hautboy or a clarinet stop, we have a peculiar tone. Now we will take the trumpet stop, and now the piccolo. See how different they are. The human voice has its peculiarities in the same way, and, as we see, the vowel sound must be analyzed very carefully, so that the number of over-tones and the peculiar pitch of them are all recognized, and in this manner we can appreciate the various qualities of sound or its timbre.

THE MECHANISM OF HEARING.

Now let us consider some of the points in connection with the reception of sound by the ear. The ear is a very curious piece of apparatus—the most complicated probably of any that exists in the body. The ear is capable of appreciating about eleven octaves of sound. The eye is capable of appreciating but one octave in color, so that the ear has a far greater range of appreciation than the eye. There are certain peculiarities about the ear by which we are enabled to appreciate these sounds; and we must not forget that our appreciation of sound—the subjective condition of sound—is, as we already see, very different from the actual condition of things or of matter which produces the sound. Sound consists of vibrations, and the ear takes these vibra-

tions by a peculiar sympathetic process, which we will understand later on, and the brain then has generated in it a peculiar condition which we understand as sound. If we irritate the nerves which go to the ear we feel no pain, but have a rumbling sound. If the irritation be even, so that the vibrations or taps of the nerve follow each other at regular rotations and equal intervals, music is the result; if irregular, noise is the result; but the acoustic nerve can transmit no other sensation to the brain but that of sound. In like manner the optic nerve can transmit no other sensation but that of sight. If it be wounded, a flash of light follows; if the acoustic nerve be touched, a sound follows. And you know there are some nerve diseases, the result of disturbances occurring in the ear, where there is a continuous sound or humming kept up, and this gives us an idea of the peculiarities of these nerves. We can appreciate, then, the subjective conditions, and we wish to study now the manner in which the sound is carried to the ear. There are two distinct sets of apparatus in the ear, one for the reception of the sound and transmission of it externally, and then the internal arrangement by means of which the sound is received and transmitted to the brain.

CONSTRUCTION OF THE EAR.

We have on the external portion of the head what is called, ordinarily, the ear. Now, this is supposed, in a human being, to be of little account so far as collecting sound is concerned, because from records we have of practices in savage countries, where the ear has been cut off, we have found that the hearing is scarcely impaired; but in the lower animals, undoubtedly, by putting the ear in various positions, they can collect the sound, and cause it to enter the interior portion of the ear. In the interior of the ear, as we pass in from this exterior portion, there is a little piece of membranous tissue, which is known as the drumhead or membrana tympani, and the waves of sound impinging against this cause it to vibrate, and the vibrations are transmitted inward, and they set in vibration, by means of a peculiar chain of little bones, another membrane, and this covers a cavity filled with fluid, and within this cavity are the filaments of the acoustic nerves. These filaments are attached to little strings, and by sympathetic vibrations these pick up the tones—not only the fundamental tones, but all the overtones which we have seen to exist in every musical sound—and there vibrate according to the intensity of the original tone produced.

This particular part of the mechanism of the ear is inclosed in a bony box, and the complicated arrangement which is here met with is enough for a life-long study. The general conditions which I have mentioned are that we have these carefully protected, that we have waves produced in a fluid, and that the undulations or waves in this fluid are the direct means by which the terminations of the acoustic nerves are set in motion, and these produce a sensation of sound. Now, a question would naturally arise as to whether or not these little membranes do not possess a fundamental tone. If this membrane were simply free, if it was simply attached around an opening with nothing else in connection with it, we would hear constantly, when a fundamental tone was sounded, this ringing in our ears; but nature has provided a peculiar arrangement which we find holds good in physics, and which you will see to be beautifully represented in the talking-machine here, namely, that it has damped, that it has checked this fundamental tone. About the center there is a little bone which is attached to this membrane, and this is in connection with several others and acts as a damper or weight. Now, if we apply this to the central portion of the membrane, it will not then respond to the fundamental tone; but by forced vibrations other tones will be transmitted, and we will not have a confusion of the fundamental tone continually drummed in our ears. And this membrane, instead of being flat, is stretched backward, and this kills, more than anything else, the fundamental tone. The result is that we have a perfect arrangement for transmitting sound.

VIBRATIONS OF SPEECH SHOWN ON THE SCREEN.

Here is a little piece of India-rubber with a little mirror in the center, and when certain tones are spoken in this tube, which is the carrying portion of the ear, we shall see that this is thrown into vibration. I will generate here an intense beam of light from the electric lamp. Now you see the light upon the screen, and if we sing a note in the tube, you will see the membrane is thrown into vibration. If those vibrations could be photographed we could analyze the number and character of the harmonics. Now we will say the words "one," "two," "three" [suiting the action to the word]; and now when we sound the vowel "u" we see also the peculiar effect produced on the screen. [Applause.] This then is one method of analysis of tone.

Now then let us follow out completely the mechanism of the production of the sensation of sound. The wave is generated, and it throws into agitation the membrana tympani, and that membrane carries it into the interior of the ear by means of a chain of bones. One of these bones comes in contact with another membrane situated deeper in, which closes over a cavity containing fluid. This fluid receives the wave impression, and as these are received the undulations are transmitted deeper into the structure of the ear, and the structures here take up the waves or vibrations and throw into agitation certain little cords, of which there are about 3,000 in the ear, and these little cords vibrate in unison or sympathy to the peculiar tone that has been produced by the sound. As they vibrate they agitate the internal filaments of the acoustic nerve, and thus the sound is transmitted to the brain. This curious part of the ear is very complicated in structure, but we shall be enabled in a moment to see the principal part where the sound is supposed to be received. [Illustrating on the screen.] There is the drum membrane of the ear, seen from its exterior surface. It is a little membrane attached to a bony structure and projecting out toward the center, where it is attached to the hammer bone. We can hear a tone which consists of sixteen vibrations in a second; we can hear a tone which consists of about 40,000 vibrations; but when we get below about forty vibrations there is produced what would be called an unpleasant sensation, which is more a noise than a musical tone. On the other hand, when we get above 4,000 vibrations there is a shrill scream. Music then consists of from 40 to 4,000, but the complete compass of the ear is from about 16 to 40,000. These little strings arranged in the ear are capable then of giving us this long variation, and not only this, but an appreciation of the $\frac{1}{16}$ part of an interval between two tones. Take for example what is called a full tone or a whole tone [touching a key on the organ]. Now the next interval of a whole tone is this [striking it]; and a half tone is very different, you see. The ear, however, can detect the difference between the $\frac{1}{16}$ part of one of these whole tones or $\frac{1}{32}$ of a half tone. In the ear these fibers are so tuned that the fiber which is next in order just vibrates with this difference of intensity. Now, in order to appreciate the manner in which these fibers vibrate—how it is that the sound is conducted from the membrane into the ear—we will make a little experiment.

PROOF THAT VIBRATIONS ARE TRANSMITTED.

I have here a tuning-fork and in front of the lantern is another. These are tuned so that the tones are in perfect unison. Near the top of one of the tuning-forks is a little ball suspended by a filament. Now if we project a shadow of this latter one upon the screen and sound the other tuning-fork—which is some fifteen feet distant—you will be enabled to see that the vibrations of that tuning-fork [the one shown on the screen] will throw the ball away from it. There is the physiology of it. [Illustrating it by the actual experiment, in which the screen showed the ball violently agitated, and repelled from the distant fork.] Now those little strings which are tuned in unison must vibrate when a sound is produced of exactly the same fundamental note.

There is just one more point before we go to the mechanism of the voice, and that is the combination of sounds pleasant to the ear and those unpleasant. I sound, for example, two tuning-forks. [Doing so.] Those tones are pleasant, but if we have the intervals too close [sounding the forks again] there is something which we don't like to hear; and we can illustrate that very readily again by taking a low tone where the vibrations are deep. [Striking low notes on the organ.] Those, you see, are not pleasant, and if you listen you will see there is an interval of silence which occurs when those two are sounded. Now, let us explain. [Turning to the blackboard.] Here we have from this point on the blackboard to this point a wave of sound. Suppose that this wave starts at the same time [drawing another] as that one does, but moves a little faster, it will make an excursion to this point [illustrating it], and then one wave will be going in one direction and the other wave will be going in an opposite direction, and when they are just in an opposite position they neutralize each other and no sound results. However, as they pass along they get into the same phase, as it is called, and the apices of each meet and then we have an intensification of the tone; and we find by bringing these two tones out of unison and sounding them together, we have a beating motion which is unpleasant to the ear, and this is known as dissonance or discord. Where there is no chance, however, for one wave to go over another, we have the sound called consonance or accord. There is no reason why this should be, except that it is a physiological fact. We can study the manner in which various combinations of tone are produced by combining the reflected images of the vibrations of the forks with each other.

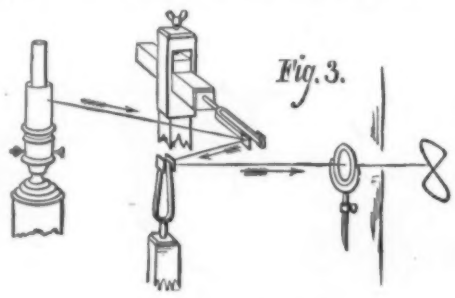


Fig. 3.

THE LISSAJOUS CURVES.

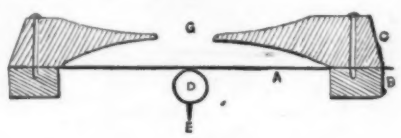
I will show you the experiments of Lissajous, where we have tuning-forks tuned to each other at different intervals, and they are each caused to vibrate so as to produce reflected images on screens. There are certain laws which these follow which can be worked out by mathematics, so that we can understand the relation of vibrations to each other. The vibrations caused by Lissajous' forks are simply conditions of the pendulum vibrations. It will simply show you the manner in which the vibrations appear when one fork is vibrated perpendicularly to the other. I will throw a beam of light from the lantern so that it strikes a mirror. The beam of light is reflected from the mirror of the first fork, which is vertical, to the second, which is horizontal. The forks are kept in perfectly even vibrations by means of electro-magnets. The forks are slightly out of tune so that we can see the manner in which these waves are following each other all the time when the intervals are not exactly tuned, and you will see a perfect figure which represents the interval, and there will be backward and forward phases in this figure known as the waves. When their crests agree with each other we have a perfect figure. When there is any difference the figure changes backward and forward in the negative and positive phase, as it is termed. Here [referring to the screen] we have a circle which is the perfect phase. Now you see that circle turned so as to form an ellipse, then the lines cross and go back again to a circle. It will take a moment or two to put in another fork, and the next one will be an octave with just double the number of vibrations. These are also slightly out of tune so as to show all the phases of this figure. Now the figure produced by the octave is very different from the other. Here we have an octave which resolves itself on the screen into the figure 8. We will try another combination, where the figure will be changed. We will take one that is somewhat complicated—that is, one fourth above the fundamental note. The nearer the notes are the more complicated is the figure. There are other intervals which can be shown, but that is not necessary.

RELATION OF VOICE TO SOUND.

Voice is produced by vibrations of what are known as the vocal cords. The vocal cords are little membranes situated in the throat, which, by the action of certain muscles, can become tense and lax, and the air which is driven out by a respiratory effort throws these little cords into vibrations, and they transmit the vibrations to the column of air. Now we have the cavity of the mouth, which contains the teeth and the tongue, the hard palate and the soft palate, and the roof of the mouth, and when we enunciate certain words we produce the vowel sounds and the consonants. We make use of the lips in producing what are called the labials. We use certain powers back in the throat for producing the gutturals, and the tongue for producing what are called the glossal sounds. All of these combinations of sounds are the result of changed forms of the oral cavity. The vibrations are set up by throwing into agitation the vocal cords, which are struck on the same principle as the reed pipe in the melodeon, the reed pipe being simply a tongue of metal over an aperture, and this throws into agitation a column of air contained in an apartment where the reed is situated. So, then, we can have in the mechanism of the voice the vowel sounds and the consonants and the guttural tones, and all of those which are not pure musical tones which are generated in ordinary conversation. By combining the two we have spoken language. If we cause the voice to throw a membrane into agitation which has any electrical connection with a battery, and if we have magnets which will represent the agitation, we will then produce what is called the singing telephone. Now I have a conversational and a singing telephone upon the table, and I shall ask my assistant, Dr. Miller, to tell me when he is ready, and when he sings in the mouthpiece of the telephone it will be heard here, not the words but the quality, which is represented by this guitar body. In the place where the strings are fastened on the guitar is a piece of wood glued firmly to the top of the guitar. Secure upon that is a soft iron plate, and the ends of this magnet are brought opposite this iron plate. Upon the other end, where Dr. Miller is, there is a membrane made of paper, and in the center is a little piece of metal, which is in the center of the circuit, and the pin which makes or breaks the circuit as the vibrations of his voice are brought against the membrane. [The tunes sung by Dr. Miller were distinctly heard, but with the quality of guitar music.]

By a combination of pipes in this organ [the large organ in the hall] there is what is called the *vox humana*, which gives a very curious effect [illustrating it on the organ]. It is simply the reed pipe, as it is called, which is shortened, according to the description given to me by one of the gentlemen who manufactured the organ. It is boxed up or confined and carried off to a great distance from the organ, and opens, in this case, in the ventilator.

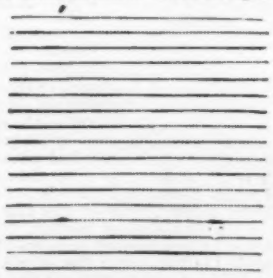
Now, if you are perfectly quiet, we will see if it is possible to have this speaking machine of Mr. Edison's recite to you something about Mary and a little lamb. [Laughter.] Now this is nothing but a mechanical contrivance. Professor A. M. Mayer has recently written an elaborate description of it, and he speaks of a celebrated talking machine constructed by Professor Faber, of Vienna. This consisted of a machine that represented all the parts of the vocal apparatus. There was a reed to take the part of the vocal cords, an oral cavity which could have its shape changed by depressing keys on a key-board, a rubber tongue and lips to make the consonants, and a little windmill rolling in the throat to make the R's. This was simply the reproduction of nature as far as possible. But there had to be a special combination in order to produce each of the different sounds. When certain sounds had to be produced, lips had to be changed in order to produce them. In the machine, however, which has been invented by Mr. Edison there is simply a piece of ferrotype plate, about 0·01 of an inch thick and 1½ inch in diameter, and it is damped and clamped between two rings. There are pieces of India-rubber pressed in between these two rings so that the ferrotype cannot sound its fundamental note, but will only respond to what are called forced vibrations. Now, upon the under surface of this plate there is a little steel point, which is attached to the plate by means of a rubber ring intervening between the plate and the steel point. [Here the Professor drew a section of it on the blackboard.]



PHONOGRAPH MOUTHPIECE—IN SECTION.

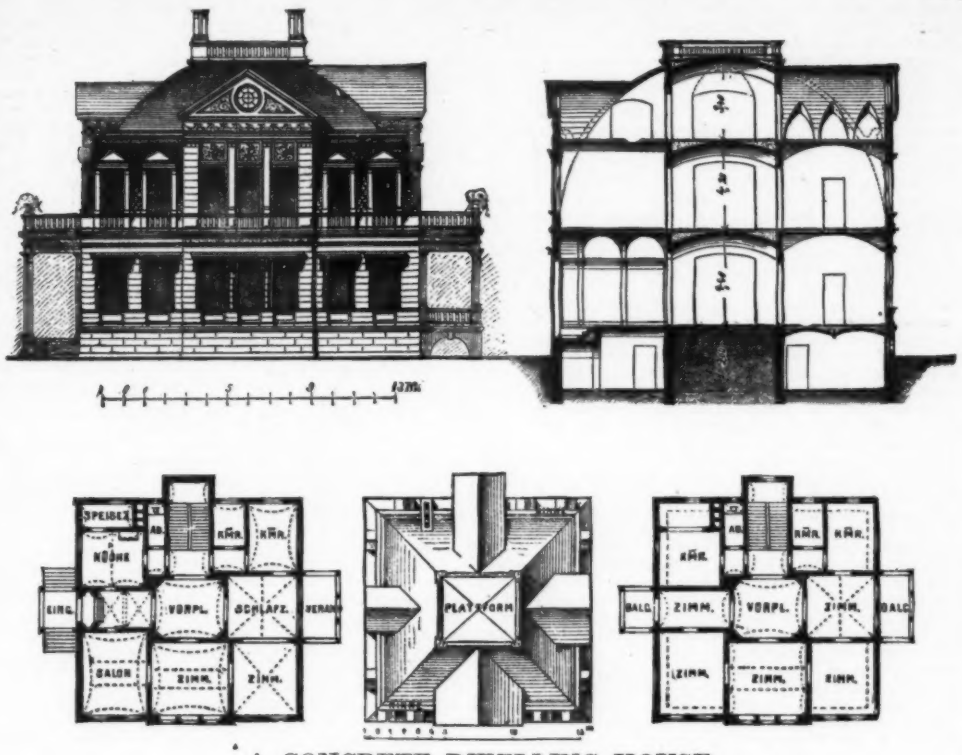
The above represents a sectional view of the mouthpiece of the speaking phonograph. A is a thin plate of iron, circular in form, about 2½ inches in diameter. This plate is placed between two brass rings, B, C, the latter being hollowed out above and below, so as to touch the iron plate at the edges only, thus leaving the latter to vibrate freely at its center. To this plate is attached, on the under surface, a small piece of rubber, D, and a fine steel point, E, is affixed to the rubber. It will be readily understood from the above that when the lips are placed at the opening, G, in the ring, C, the vibrations of sound produced in speaking will be communicated to the iron plate, A, thus causing the steel point to be depressed and raised, these motions succeeding each other with a rapidity depending upon the rate of vibrations in a given tone of the voice.

The only portion of that plate that can vibrate is about half an inch across. That is the portion which we have here [indicating it]. Underneath the plate there is a piece of rubber ring cemented to the plate. The cylinder which we turn by hand is grooved into a spiral thread, and this little point impinges in the depression so that it does not touch the plate. As we turn it around, we would trace the spiral on the whole of this surface, which is covered over with tin-foil, and there is a screw-thread cut in the axle, so that when we turn this machine it simply describes a spiral on the surface of the cylinder. All that it is necessary to do to make the machine talk is to put on a piece of tin-foil.



SPEECH TRANSCRIBED ON TIN-FOIL.

The needle simply is to make a very light groove, and then you talk against this plate or through a paper cone of this construction [exhibiting it]. Now we have simply to reverse the motion and turn the crank again in the same way as it was at first, and we have repeated what was said. The loudness is not very great yet, as the invention is not yet perfected. When the plate is thrown into vibration the point dips down and makes indentations in this tin-foil. Now those indentations correspond to the peculiar form of wave-sounds which have been uttered by the mouth. The consequence is that the plate is made to vibrate in this direction, as you see.



A CONCRETE DWELLING HOUSE.

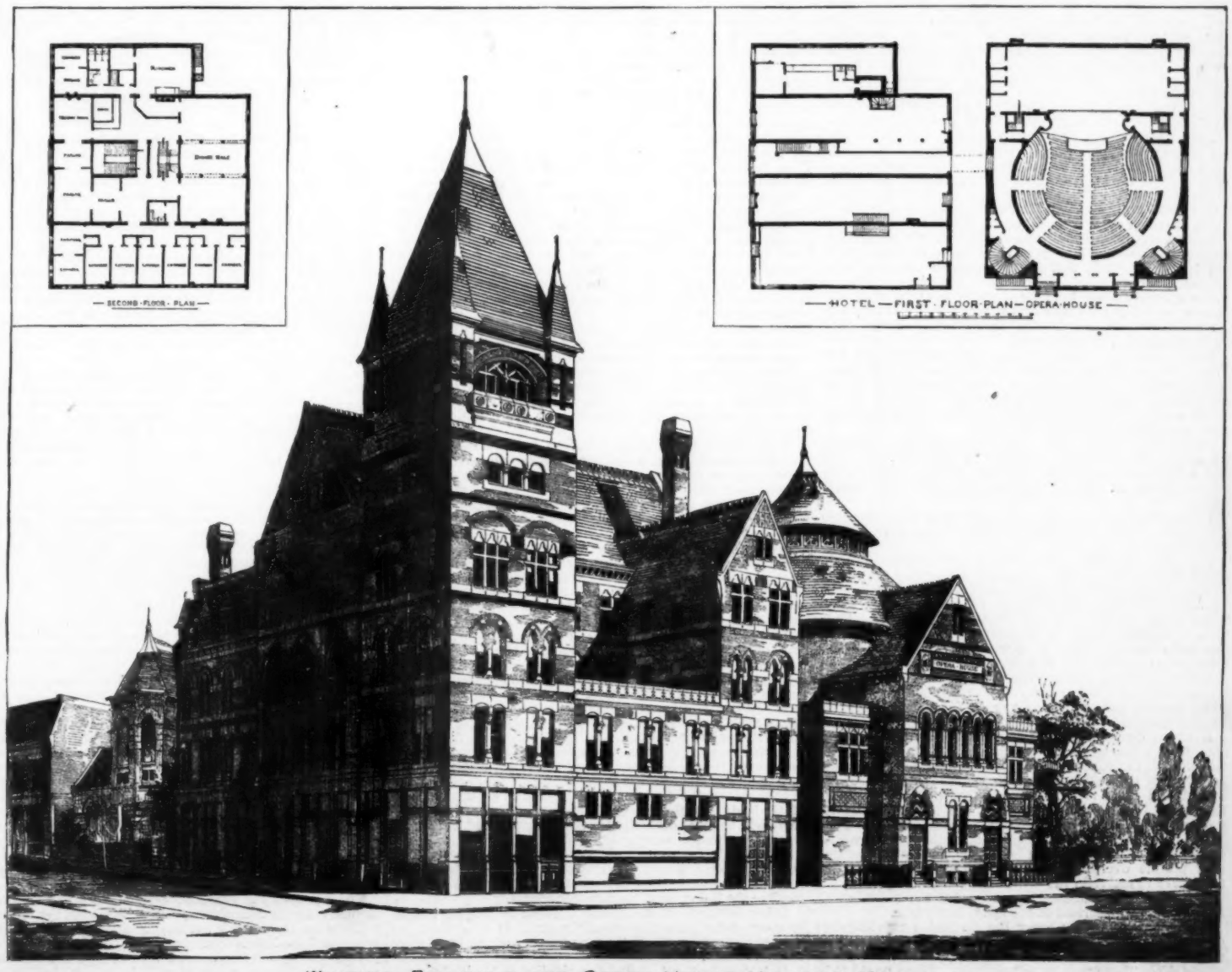
THE WHITING BUILDING AND OPERA HOUSE, HOLYOKE, MASS., MR. C. S. LUCE, ARCHITECT.

THESE buildings were erected for William Whiting, Esq., the present Mayor of Holyoke, and are now rapidly approaching completion. The hotel building is constructed of Philadelphia pressed brick, with finish of light Nova Scotia sandstone, and comprises stores on the ground floor, dining hall, kitchen, parlors and chambers on the second and third stories and a public hall on the fourth story. The tower is about one hundred and fifty feet in height. On the exterior of the opera house, light Philadelphia and dark Holyoke pressed brick are used; and bands of black brick and panels of majolica tiles are introduced. The central gable is further ornamented by two circular panels containing heads of Comedy and Tragedy. The auditorium includes an orchestra, parquet circle, and one gallery, and has a seating capacity of about eleven hundred. It is finished throughout in the Neo-Grec style. The plan is circular. The ceiling consists of a large covered cornice, pierced by eight semicircular lunettes (forming a series of furred vaults), surmounted by a flat dome, and is ornamented at the center with a *rosace*, which serves as a ventilator to the auditorium, and from which depends a large brass chandelier. All the ornamental work is executed in papier-mâché. In the two prosceniums, though merely decorative features, an attempt has been made to render them essentially architectural. The entire theater is being decorated, and will be ready for dedication in a short time. The cost of the two buildings, exclusive of the land, will be $125,000.—*Am. Architect and Build. News.*

A CONCRETE DWELLING HOUSE.

MR. LIEBOLD, an architect of Holzminden, has published in the *Deutsche Bauzeitung* an account of a dwelling house of concrete, lately built by him in Vorwohle for a gentleman engaged in the manufacture of Portland cement. Although the house (see illustration) was quickly built, it yet seems to contain a *tour-de-force* of almost every constructive form for which concrete can be employed. The rooms are covered with various kinds of vaults, many of which have a very considerable span. Over the vestibules of the different stories—spaces thirteen by seventeen feet—three vaults are superimposed without the use of iron, and depend solely upon the vaults of the adjoining rooms for a counteraction of their side thrust. The original and striking feature of the building is the great cloister-vaulted roof, which, resting on the four principal corners of the structure, rises through a story and a half. At its base the concrete of which it is formed is one foot in thickness; at its summit only from four to five inches. It is to be regretted that it has received no architectural expression on the exterior; the objectionable mansard is excused by Mr. Liebold on the ground that it was desired by his client. The outer walls of the house are one foot thick; division walls and partitions being from eight to ten inches thick. In the cellar these dimensions are increased by four inches. The walls are anchored at suitable points, and were built above ground by means of adjustable wooden boxes, into which the cement is poured. Below ground the cellar and foundation walls were cast in trenches, the cellar itself not being excavated until after their hardening. The trenches, consequently, were dug to the depth of the cellar below the level of the earth, plus that necessary for the foundation below its floor. This total depth must have been between seven and eight feet, and can only have been obtained in firm soil.

The concrete employed was composed of one part cement to seven and four parts respectively of gravel and sand. The stairs have treads of slate, and are cast so as to measure on the string four inches at the re-entrant and seven inches at the outer angle of each step. In them a coal-slag was substituted with good effect for the gravel; the weight of the concrete thus prepared being but one-half to one-third that of the concrete made with stone. The cornices, window-casings, steps, etc., were formed of three parts of sand to one of cement, and were ready to be walled, or rather cast, into the walls, as the building advanced. The entire cost of the house, says the *Amer. Arch. and Build. News*, which is in effect a cube with a side of between fifty and fifty-five feet, was $4,300 gold. From the itemized account it appears that all the interior vaulting cost $420; the stairs, $82; and the roofs and the platform, $438, all in gold. The cost of concrete walls, including the wooden forms, etc., was 13½ cents per cubic foot. The building was completed in four months, and its construction may be regarded



WHITING BUILDING AND OPERA HOUSE, HOLYOKE, MASS.
MR C.S. LUCE, ARCHITECT.

as thorough in every respect. Immediately after it was roofed in, the inspector of buildings ordered the floors throughout the house to be weighted with sixty-five pounds to the square foot. This great weight did not produce the slightest hurtful effect on the vaults, not even a crack in the plasterings. As a preliminary experiment, a pair of concrete vaults were built last year in Vorwohle over a space thirteen by fourteen feet, the length of which was divided into two equal parts by an iron I beam; the walls, also of concrete, were twelve and eight inches thick. The center of this vault was weighted on a space containing thirty-one square feet with a weight of five tons, that is to say, over 330 pounds to the square foot, yet there was no displacement or crack either in the walls or vault. As a further example of rapid construction, a bridge was built of concrete, and prepared for use in six working days, which was twenty-four feet long and twelve feet wide; and a smaller concrete dwelling house, forty-three feet by thirty feet, was entirely completed in seven weeks. It is to be hoped that the official experiments now being made by the Polytechnic of Brunswick upon the strength and reliability of concrete will soon be made public, as their results bid fair to be surprising.

LECTURES ON PARALYSIS AND CONVULSIONS AS EFFECTS OF ORGANIC DISEASE OF THE BRAIN.

Delivered at Bellevue Hospital Medical College, N. Y.

By C. E. BROWN-SEQUARD, M. D., ETC.

LECTURE VII.

GENTLEMEN:—In the preceding lectures I have tried to show that, though the symptoms are exceedingly variable in brain disease, we can generally come to the diagnosis of the seat of the lesion. To-day I shall continue on the same subject. What remains to be examined is what relates to the convolutions of the brain. Certainly they are parts of the cerebral structure that produce exceedingly variable symptoms. We may have a great deal of difficulty in coming to a conclusion as to the seat of the lesion, but still there are some points that may lead us to a correct diagnosis.

Before coming to the symptoms produced by disease there is one point which I have already mentioned many times, but which I must now again revert to. This point is as to whether the convolutions contain the psycho-motor centers. Within the last five or six years there has been a great effort in Germany, England, France, and in this country to determine and prove this point. There has been a strong effort to show that certain parts of the convolutions of the brain are places in which the will power acts to produce voluntary movements. The parts in front of the fissure of Rolando and behind and near the Sylvian fissure are the parts where the will power acts to produce voluntary motion. The facts on which these views are grounded are extremely interesting, and one discovery by Fritsch and Hitzig possesses a great degree of interest, and deserves more than ordinary attention.

It was believed that the brain substance, at least in the central portion, was not excitable by any of the means of irritation that we possess—galvanism, mechanical irritation, such as tearing up or pricking the nerve tissue, chemical irritants, such as the mineral acids, etc., and thermic irritants, such as the hot iron. As you well know, when we apply such irritation on a nerve or the spinal cord, or on the base of the brain, we may produce reflex or direct movements if the part irritated is a motor part. No doubt if we found that any of these agents, by irritation of a central part of the brain, would always produce the same kind of movement, it would be established that that part of the brain is excito-motor, or has a direct power of producing that certain movement. This, however, is not so. The only agent that will produce such an effect is galvanism.

Therefore there is a radical difference between these and other parts of the brain, and, as you well know, galvanism can have a diffuse effect whenever it is applied. Its effects may certainly be propagated from the point to which it is applied to other parts at a distance.

This is a way of explaining the effects observed when these portions of the brain are excited by galvanic irritation, if you refuse to admit that they are centers presiding over voluntary motions.

Again, experiments have been made to show that galvanism applied to the convolutions is propagated, and that the movements which take place under this irritation only occur sometimes and not always.

There is no doubt, from the labors of certain friends and pupils of mine, led in a measure by myself—although I was mistaken—there is no doubt that local galvanization of the convolutions may be followed by certain muscular movements, but this is no proof whatever that the part thus acted upon is a psycho-motor center.

To use an old illustration, if we tickle the sole of the foot we may produce certain muscular movements of the face that we call laughter, but we certainly do not look upon the sole of the foot as the psycho-motor center for these movements.

The same reasoning applies with full force when we consider the movements produced by irritating the convolutions of the brain.

There is no reason to conclude that the irritation acts directly on a motor organ. A motor center may have been excited, but it has been through a propagated influence, and a reflex action is the consequence. It is, I repeat, the same thing as occurs when we tickle the sole of the foot. This view is certainly more in harmony with the facts than the one generally admitted. If it were the case that a motor organ is excited by the irritation, all irritants, as well as galvanism, should produce the same effect. Moreover, if these parts were taken away, we should get paralysis of those muscles that act in causing the contraction, and this paralysis would be persistent.

In animals, when these parts are taken away, an appearance of paralysis will come on, but when we investigate this we find that the apparent paralysis is similar to the general paralysis of the insane, in which the convolutions are inflamed, and there is irritation of other parts of the brain tissue. It is simply a disorder in the action of the will power, and not paralysis, that is present in these cases. When these parts are taken away, and such results are seen to follow, it would show, if it led to any conclusion, not that they preside over the voluntary motor actions, but that they serve to maintain the equilibrium.

The second conclusion, however, that the convolutions maintain the equilibrium of the body, is not true.

If, instead of taking away the pretended psycho-motor center alone, we remove a great deal more of the tissue, we ought to have certainly no power of producing these movements left. There may be no paralysis. There ought to be a greater paralysis, but in many cases there is not only no paralysis at all, but not even the appearance of it. If you only take a part of the organ away you will have more disorder of movements than when you remove the whole of it. The effects that are seen, then, can only be the effects of irritation.

Longet found in one case that, after producing apparent paralysis on one side of the body by taking away the so-called psycho-motor centers, when he took away the corresponding centers on the other side of the brain, the paralysis produced by the first operation disappeared. He should unquestionably, if the theories were true, have found a second paralysis on the opposite side, and no change whatever in the side that was first paralyzed. This is decisive in showing that an irritation is produced in another part of the nervous system by the first injury. By the second operation you produce cessation of the apparent results of the first.

Professor Bayer, of Paris, experimented on a chameleon by removing the brain on one side, and the result was paralysis of the other side of the body. He then took away the other half of the brain, and, instead of producing the same condition on the sound side of the body, the paralysis that appeared from the first removal disappeared. In this case one-half of the brain had been removed, and the paralysis produced appeared to be due to the fact that the voluntary motor centers had been taken away, and the second operation, removal of the remaining half of the brain, instead of being followed by a second paralysis of the other half of the body, was followed by the cure of the animal of the first paralysis, that produced by the removal of the first half of the brain. This is is a mode of treatment, however, that I should not advise in the human subject.

From the facts that I have related it follows that there is no reason to conclude that paralysis depends on the loss of an organ employed by the will power. The reality is that when paralysis appears it is due to an irritation which starts from the place in which the disease is situated, and from that point spreads its influence to many others in the cerebro-spinal system, and causes, by an inhibitory influence, a cessation of the activity of those cells to which it has spread, and thereby produces paralysis.

The results of all the experiments with galvanism tend likewise to show that paralysis may be produced by irritation in this way.

Bouchfontain found that the application of galvanic stimulus to the dura mater, or even simple gentle friction with the nail, according to where it was applied, could produce a movement of either the arm or leg. From this fact are we to place the center of will power for movement of the leg or arm in the dura mater?

Even those persons who believe in the existence of the psycho-motor centers admit that paralysis, apparently due to their removal, disappears after a time. It is therefore plain that in these cases we have to deal with something else besides the ablation of an organ.

A great effort has been made by Prof. Charcot to show that certain conclusions as regards the motor centers must necessarily be drawn from clinical facts. He has shown that certain parts of the convolutions of the brain have more power in producing paralysis than others. There is no doubt that certain parts around the fissure of Rolando, when they are subject to disease, will give rise to paralysis more frequently than when the disease is in other parts of the convolutions; but if you examine the facts you will find but very few which are in harmony with the admitted theory.

According to these observers, the anterior convolutions contain centers that move the tongue and lips, and those in front of and behind the fissure of Rolando serve chiefly for the movements of the arm and leg. These parts, and some others that it is not necessary to mention more particularly, along the central fissure of the brain, are the so-called psycho-motor centers.

We find many cases in which disease has destroyed these parts without the production of any marked paralysis. These facts are certainly quite sufficient to show that the conclusions are wrong.

If you examine the facts seemingly in harmony with the theory, you will find in many cases where the disease has occupied only small parts that there still may be complete hemiplegia. In other cases we find that the face is paralyzed together with the arm when the lesion is situated in that part of the brain considered to be the motor center for the leg. Indeed, the discrepancies between the cases and the conclusions of physiologists from their experiments are decisive against the theory.

Is it possible, however, to diagnosticate a disease existing in the convolutions of the brain, even when the disease is elsewhere than in the psycho-motor centers, so-called?

Sometimes it is so. Disease somewhere else may produce paralysis of the same kind, but we must endeavor to look as carefully as possible for the seat of the disease, as the means of treatment vary according to the location of the lesion.

If the disease is in the convolutions, together with paralysis, there is a likelihood that convulsions will occur. Convulsions occur more frequently when the disease is in the convolutions than when it is in any other part of the brain; so much so that Dr. Wilkes, of Guy's Hospital, thought that epilepsy was due to disease in the cerebral convolutions; and I made an equal mistake when I considered that it depended always on disease of the membranes outside or inside of the ventricles. There were a great many facts in favor of my view, but also a great many against it. However, in many cases where the disease is situated in the convolutions, it is likely that we will have convulsions with the paralysis. So then when we have convulsions present in such cases we may suspect that the disease is in the convolutions, and examine carefully for further evidence.

With disease here, it is frequent to find disorder in movements, if movement remains. Nearly always there is not simply loss of voluntary action, but also disorder of movement. Then, besides, the paralysis is usually much less in extent than in disease in other parts of the brain, and the paralysis is chiefly marked in the arm.

Again, the convolutions of the brain are employed in the exercise of the noblest faculties of the mind, and therefore there will be some disorder in the higher faculties when they are the seat of the disease. If the lesion is on the left side of the brain, whether or not it be in the third convolution, there will be great difficulty for the patient to find the proper words by which to express himself.

Amnesia is frequent, and very frequently there will be complete loss of power of expressing ideas by speech. So that there are a number of symptoms that may lead us to localize the lesion with considerable accuracy.

Other features are interesting. There is frequently contracture of one limb, oftenest of the arm. If the disease has existed for any length of time, as from a tumor, this is more likely to occur.

It is very frequent to find this rigidity in the limbs, especially in the arm.

Prof. Charcot tried to explain this on the ground of secondary degeneration. That portion of the convolutions in front of the fissure of Rolando is the part, disease in which chiefly produces the rigidity. Charcot showed that the secondary degeneration extends from this situation to the pons Varolii, the medulla oblongata and the spinal cord.

The convolutions of the brain have also peculiar features that may help us in making a diagnosis. My friend and former assistant, Dr. J. Hughlings Jackson, was the first to clearly show the features that usually belong to these cases. If epilepsy or convulsions appear as the result of the disease in the convolutions, the arm especially, but sometimes the leg, will be seized by a cramp before the attack.

Sometimes with these attacks there are peculiar sensations, as formication, or burning or pricking sensations, and for a while the symptoms are localized in one limb only.

These features are not absolutely peculiar to disease in these parts, but certainly belong far more frequently to lesions here than to lesions in other parts of the brain.

I cannot review all the symptoms which will lead us to the diagnosis of the lesions that produce paralysis, but I will now repeat in as few words as possible the reasons that lead us to localize the disease.

If you find paralysis in the limbs, with paralysis of the face on the opposite side, you may look to the pons Varolii as probably being the seat of the lesion.

If you find paralysis with hemiopia, there is likelihood that the disease is in the tubercula quadrigemina.

If you find paralysis coexisting with complete loss of action of the third pair of nerves, and paralysis of the limbs on the opposite side of the body, then the disease is probably in one of the crura cerebri.

These are the most important features.

I now pass to another point—the study of convulsions in connection with brain disease. Convulsions, as you know, may appear without epilepsy. They are certainly distinct from epilepsy proper. What essentially characterizes epilepsy is loss of consciousness. There is, indeed, no need for convulsions to be present in attacks of epilepsy.

In the *petit mal* of the French, or epilepsy *mitior* of the English, there is no necessity of convulsions or rigidity of any muscles in the body. There may be a simple loss of consciousness from many causes, but for epilepsy there must be a few such attacks, as loss of consciousness itself cannot constitute epilepsy. When loss of consciousness occurs from arrest of the heart, we have syncope, but when the heart continues to beat, and there have been a number of attacks of loss of consciousness, then we have epilepsy. But I have seen very few cases in my experience in which there were not convulsive movements of some muscles, especially of some of the muscles of the face or neck. There may be such cases, but epilepsy is usually characterized by two series of manifestations, loss of consciousness and two kinds of convulsions. The first is rigidity or tonic spasms, almost always coming at the commencement, and, second, the other series of movements or clonic convulsions.

These usually exist in succession, the tonic convulsions preceding the clonic. These varieties occurring in succession, in repeated attacks, with loss of consciousness, constitute complete epilepsy.

In brain disease very frequently you have not to deal with epilepsy proper. Convulsions very frequently occur without loss of consciousness in such cases.

Other symptoms characterize convulsions dependent on brain disease, as compared with those occurring in idiopathic epilepsy. In the latter case the convulsions are almost always alike on both sides of the body.

In convulsions dependent on brain disease they are very rarely alike on both sides. The head is drawn to one side, and the eyes are moved to one side, and there is a difference in the limbs on the two sides. Another feature is that one side of the body alone is frequently attacked with convulsions in disease of the brain, but in epilepsy this is never the case. Convulsions, then, occurring on one side of the body lead to the suspicion that disease of the brain is the cause, especially when the convulsions are limited to one limb, as the arm or the leg.

In cases of convulsions due to brain disease, what was called by Galen and other physicians of his time an *aura* will occur. Another feature of epilepsy due to brain disease is its curability. I may surprise many practitioners in this room when I say that epilepsy proper, or simple convulsions due to brain disease, generally can be cured, while on the other hand hardly one case in a hundred of idiopathic epilepsy can be cured.

So you see that there is a radical difference. It seems strange that in such cases, that seem to be the more aggravated conditions, our means of cure are more certain.

When epilepsy depends on disease in special parts of the brain, as in the base, our means of treatment may cure the case rapidly; that is, we can prevent the manifestations of the disease in its commencement, and by so doing we can cure it in time.

The patient may have subsequent attacks within two years, but each attack may be averted, so that he is completely cured only after some time; though, as I said, the manifestations are prevented.

There is no doubt that epilepsy from brain disease depends simply on an irritation of certain parts of the brain on which our means of treatment exert a powerful and controlling influence.

It may be that before a very long time has elapsed we shall have the same great power over paralysis as we now exert over convulsions, as, according to the theory I have set forth, it is only a manifestation of irritation, in the same way as convulsions.

An irritation starts from a certain point and produces an inhibitory effect, a cessation of activity on cells at a distance, and paralysis results.

If the irritation goes to cells that are able to produce reflex action, instead of to simple motor cells, another irritation is produced, and convulsions occur. It is essentially the same cause in both cases. An irritation starts from a certain point, is propagated to distant parts, and produces paralysis or convulsions, according to the properties of the part on which it acts.

If we can cure convulsions dependent on brain disease, we may find means to cure paralysis as well, for they are both dependent on the same cause. What has already been discovered as regards anæsthesia is most important as regards our being able to cure paralysis.

I now pass to the study of certain features that present themselves with convulsions, due to disease in the brain.

These are the *aura* or warning symptoms that precede the attack and the loss of consciousness.

First as regards the *aura*, I wish every one would study with great care every case of epilepsy to see whether or not this exists. It is true that it is very difficult for a busy

practitioner to examine every part of the entire surface of a patient's body, but not to do it is a lack of employment of our knowledge, and we do not do our whole duty to our patient if we neglect it.

I know that I myself have been guilty of such neglect.

I will state the facts that have led me to these remarks. I have found that irritation of certain parts of the body in epileptics will often and in some cases always produce an attack of the disease. I have not dared in many cases to produce an attack by such means, but in a few I have. I never wish intentionally to produce an attack, but it is important in some cases to ascertain whether we have to deal with epilepsy due to organic disease of the brain or to some other cause.

In this way we bring the attack to be like those that are preceded by an *aura* appreciable to the patient. If there be an *aura*, and the place where it starts from is determined, then the application of a variety of means of counter-irritation has a great power of curing the attack. So if you produce an attack of convulsions in this way, the patient is well repaid, as counter-irritation applied to the part may produce a cure of his disease.

This spot may be situated in any part of the body, so that an irritation applied to the back of the head, or neck, under the ear, or under the jaw, at the level of the origin of the vaso-motor nerves, the first or second dorsal nerves, the knees or other parts of the limbs, in some cases the breast, and so on, may be the means of curing the epileptic seizures. Abercrombie and Bright had cases like these. Reeves Clark had a most important case in which a touch on the breast produced an attack. In these cases counter-irritation applied to these points may effect a cure.

The *aura* may vary greatly. It may consist in a peculiar movement or in a sensation; the movement may occur anywhere in the body.

I had a case in London in which there was such a violent contraction of the bladder that the urine was forced out with great rapidity. This occurred two or three hours before the attack. Any muscle in the body may contract in this way. The organs of the abdomen or chest may be affected in like manner. Any muscle of the limbs or trunk may contract in the same way as did the muscles of my patient's bladder. As you well know, there are muscular fibers surrounding the blood-vessels. On this account you will get a pallor of the part in which the contraction occurs. It may be in the face or elsewhere. There is likewise a diminution of the temperature of the part.

There is, therefore, a kind of *aura* which consists in muscular contractions in various parts of the body. The bronchi, the diaphragm, the intestines, in fact any part may be subject to this contraction. The important point, for the sake of applying means of treatment, is to determine whether such a place exists, and if it is a constant place, and if the *aura* frequently occurs there previous to an attack. If such a place can be found, you have a great chance of doing a good deal for the patient by applying means of counter-irritation.

Another kind of *aura* is one of sensation. It is variable, but it generally consists of an indefinable sensation. The patient cannot tell what it is like. Its peculiarity is that it is like itself and nothing else; in fact it cannot be accurately described. It belongs only to that peculiar state. Some physicians think that the term *aura* should be limited to this peculiar sensation, but the term should apply to everything that takes place before the beginning of the actual attack.

In those cases in which the *aura* appears long before the attack, we have great chances of staving it off.

I pass now to the consideration of loss of consciousness. In 1857, in this country, in a work on epilepsy I tried to show, what is now admitted by physiologists and physicians, that the loss of consciousness is the result of the contractions of the blood-vessels in the cerebral lobes.

I am sorry indeed that my ideas were followed, for I now believe that it is only partly true, if true at all. I am quite certain that the blood-vessels contract, for I have seen it many times, and the fact is beyond dispute, but, just as in the case of sleep as shown by Dr. Hammond, the contraction is not essential. To say a few words about sleep, the contraction of the blood-vessels is so little essential to that phenomenon that in animals in whom both sympathetics have been divided, and where there can be no contraction, but on the contrary a state of congestion exists, sleep takes place just as well as if the blood-vessels of the brain were contracted.

It is clear, therefore, that contraction of the blood-vessels of the brain is only something that takes place during sleep, and is not the cause of sleep; so that a remedy that produces contraction, given in order to produce sleep, is wrong. The contraction of the blood-vessels is not essential; it may be accidental. I will consider in the next lecture the cause of loss of consciousness in epileptic attacks.

SNAKE POISON.

EVERY now and again the British public is horrified by accounts of the famines which periodically carry off myriads of our fellow-subjects in India, but comparatively few have the least idea of the enormous destruction of human life which occurs there from the ravages of wild animals and venomous snakes. In a most interesting lecture recently delivered at a meeting of the Society of Arts by Sir Joseph Fayrer, the lecturer estimated the loss of life at no less than 20,000 human beings and 50,000 head of cattle annually. Wild animals destroy most of the cattle, but venomous snakes kill more human beings than all the wild animals put together. The bites of these reptiles caused the death of 17,000 persons and over 3,000 cattle in the year 1875, and these figures very probably understate the facts, as the returns upon which they are based are incomplete. The desirability of obtaining an antidote to snake poison is thus evident, and many attempts have been already made to discover one. Another has been added to the already numerous investigations on this subject by Mr. Pedler, who has lately published the results of his research in a paper read before the Royal Society. Before proceeding to seek for the antidote, he endeavored to analyze the poison chemically, and thus discovered several facts of great interest. The venom of snakes seems to contain very much the same proportion of solids at all times, even under such different climatic conditions as during the wet and dry seasons. It may be kept for two or three months without alteration, but if preserved for a year or eighteen months, it becomes insoluble, and, to a great extent, loses its poisonous qualities. Its composition is very like that of albumen, and, indeed, the dried poison, which looks very like gum arabic, contains about sixty per cent. of albumen, and only forty per cent. at most of the poisonous principle. By the use of solvents, Mr. Pedler endeavored to separate a crystalline principle, such as Lucien Bonaparte affirmed to be present in the poison of the rattlesnake. His attempts were unsuccessful, and he therefore tried to obtain it by dialyzing the poison through parchment paper. Part of the poison dialyzed, and part did not. On evaporating the fluid inside the dialyzer, the residue formed a gummy mass, with a poisonous action. The water outside the dialyzer also gave a similar result, but in it a few crystals could be detected. It was, if anything, rather more poisonous than the ordinary virus. He did not succeed, however, in obtaining any very definite crystalline substance. Ammonia, which has lately been highly recommended as an antidote in snake poisoning, he found, as did Fontana two hundred years ago, to be useless, and indeed its addition to the poison before injection seemed really to hasten death.

Some organic poisons may have their physiological action greatly altered by changing their chemical constitution. Thus strychnia has its action completely altered by combination with iodide of methyl, so that instead of producing convulsions it causes complete paralysis, like curara. At the same time its deadly power is greatly diminished, and it occurred to Mr. Pedler that the poisonous properties of cobra virus might be diminished in a similar way. On testing this supposition he found it to be correct, as the poison, after digesting with ethylic iodide, took five times as long to kill an animal as fresh cobra poison would have done. Hydrochloric acid also diminished the activity of the virus, and platinum chloride had a still more powerful action. This salt seems to combine with the poisonous principle of cobra virus, forming with it a yellow amorphous precipitate, which is very insoluble in water, and which has little or no poisonous action. This result of the action of platinum chloride on cobra virus out of the body is most satisfactory; but this apparent antidote has not the same power when the poison has once entered the system. When the poison is injected under the skin and the platinum chloride is injected shortly afterward into the same spot, death appears to occur even more quickly than when no antidote whatever is used, the second injection seeming to drive the poison before it and to cause it to act more rapidly. When the platinum chloride, however, is injected at the same point but somewhat more deeply than the virus, so that in passing inward the poison might come in contact with the platinum, life is considerably prolonged. If a short time elapses between the injection of the poison and that of the platinum, death ensues, even though the interval be only one or two minutes. It would thus seem that when the platinum chloride is brought directly into contact with the poisonous principle of the cobra venom it renders it insoluble and prevents its poisonous action, but that it is not a physiological antidote, and will not counteract the deadly action of the virus after it has once entered the circulation. It may be useful as a local application, but cannot be regarded as an antidote. Every means hitherto tried of counteracting the effects of cobra venom has thus proved ineffectual. Artificial respiration, proposed by Sir Joseph Fayrer and Dr. Lauder Brunton, gave fair promise of success, and by its use the heart may be kept beating for many hours. Indeed in one case an animal apparently dead for many hours has been partially revived by it, yet on no occasion has a fatal issue ever been averted by its use. The experiment just mentioned was performed by a commission appointed by the Indian Government, at Sir J. Fayrer's suggestion, to examine into the modes of preventing death from snake bite. A dog was bitten one afternoon by a water snake, and apparently died about three o'clock. Artificial respiration was at once commenced, and the heart continued to beat, but the animal seemed to be perfectly dead, and the limbs no longer responded to electrical stimuli. Early next morning, however, an alteration took place. The limbs again answered to electricity, voluntary movements occurred, and the eyelids closed not only when the eye was touched with the finger, but when the hand was simply brought near it. This showed that the animal could see the approaching hand, and closed its eyes in order to protect them from the expected touch. The dog seemed to be in a fair way to recovery, but about noon it began to get worse, and finally died at three o'clock on the second day, twenty-four hours after its first apparent death.

Whether a combination of artificial respiration with other appliances may yet enable us to prevent death altogether, is a question which can only be determined by a continuance of those experiments which led to the use of artificial respiration alone. But however valuable such a method as this may occasionally be in saving the lives of English officers, government officials, or persons living within reach of skilled assistance, and who might otherwise be doomed to certain death from the bite of a cobra, it is obvious that it is too complicated to be of much service to the numerous natives who are bitten in localities where no other assistance can be had than that of their comrades, equally ignorant with themselves. If any great diminution is to be effected in the frightful mortality annually resulting from the bites of venomous snakes in India, the remedies must either be so simple and easy of application that they can be used by the most ignorant, or the snakes must be destroyed. The best instructions yet given for the treatment of persons bitten by poisonous snakes are contained in Sir Joseph Fayrer's magnificent work on "The Thanatophidia of India." He recommends that a tight ligature be applied to the limb above the bite, that the bitten part be cut out as quickly as possible, and that the wound thus left be cauterized with a hot coal or hot iron, or touched with nitric or carbolic acid, while brandy or ammonia should be administered internally. Even this treatment, simple though it be, requires knowledge, as well as instruments and skill, which the majority of the natives do not possess. Sir Joseph Fayrer therefore recommends that in every police station and public place plain directions should be printed and hung up, and that at all such places a supply of whipcord, a small knife, a cautery iron, and a bottle of carbolic or nitric acid should be kept, as well as a supply of liquor ammonia for internal administration. But, as Sir Joseph Fayrer says, although comparatively little is to be expected even from this rational mode of treatment, much may be anticipated from prevention, and it is to be effected by making known the nature and appearance of the venomous as distinct from the innocent snakes, and by offering rewards (to be judiciously distributed) for the destruction of the former. The differences between many of the non-venomous and the venomous snakes are not known to the natives, and it is important that a knowledge of such distinctions should be widely disseminated, not only that the venomous ones may be more easily recognized, and thus avoided or destroyed, but in order to prevent death or serious illness from sheer fright, which may frequently result from the bite of a non-venomous species. For this purpose it would be well if the pictures of the chief venomous snakes contained in Sir Joseph Fayrer's work, or cheaper but accurate lithographic copies of them, were displayed in every police station and public place throughout India. Rewards should be paid for the destruction of venomous snakes only, and if these pictures were exhibited in the way suggested there would be little or no excuse for any mistake, either on the part of the natives who killed the snakes or the officers whose duty it would be to pay the reward. As to the amount of reward, and its mode of distribution, there should, he suggests, be a department, or branch of a department, with a responsible chief and subordinate agents, for whom certain rules should be laid down, to be observed steadily and without hindrance throughout the country, leaving much, as to detail, to the discretion of local authorities. If the destruction of venomous snakes and wild animals in India were intrusted to an officer such as controls the Thuggie and Dacoitee department, he considers that the result would in a few years be as good in the case of noxious animals as it has been in that of noxious men, Thugs and Dacoits.—*Nature*.

LABORATORY EXPERIENCES ON BOARD THE CHALLENGER.*

By J. Y. BUCHANAN.

THE laboratory was 10 feet long, 5 feet 8 inches broad, and 6 feet high, and although the dimensions seemed small, there was practically quite room enough for one worker. It was situated in the middle of the ship on the main deck, on the starboard, *i. e.*, the right-hand side looking forward. A locker, 2 feet 6 inches high and 2 feet broad, ran across the whole of the front side. Supposing that the visitor takes his seat on this locker looking aft, the following description will convey a rough idea of the interior: Immediately on his left is a gun port which, in the absence of the gun, provides light and ventilation for the room. Some light is also obtained from windows in the deck. The locker contains the stores of glass and porcelain, a balance, microscopes, etc. On the right is the blowpipe table; a spirit lamp, formed of a wide mouth stoppered bottle and having a large wick, was found to yield the most convenient flame. A tallow lamp was abandoned on account of the trouble of melting the tallow. On the left-hand side, further aft, is seen the principal working bench, 3 feet 10 inches high, furnished with drawers containing the reagents, each bottle in a separate compartment. On the top of the bench battens are nailed, so that the bench presents the appearance of the frame of an ordinary door without the panels; into the places where the panels should be the drawers of reagents can be fitted while in use, and so are prevented from shaking down. Ordinary retort stands were found to be useless, and the following stands were used: A half-inch metal rod was taken and a long eye cut out at one end (like a needle). Into this eye fitted a staple driven into the joists forming the roof of the laboratory. By this arrangement the retort rod had a certain play up and down. The retort rings having been slipped on from the bottom, the rod was lifted and then pushed down into a hole made for the purpose in the teak bench. This plan was found very convenient, and when not in use the rods were turned up, the eyes forming a joint, and hooked along the roof. For evaporating, drying, etc., a cast-iron frame set in gimbals and fixed to the side of the laboratory was found quite sufficient, even during the most violent movements of the ship. In the further left-hand corner are the sink and the diluted water vessel. Along the further end are wooden shelves with holes for various pieces of glass apparatus. Many vessels, etc., were slung from and between the beams supporting the deck. On the right-hand side are seen the bottles of standard solutions with burettes; the latter were made like pipettes, and filled by sucking up the solutions from below. In this way barium hydrate solution was found to keep its quality for two to three months, though used every day. On the same side the specific-gravity apparatus, hydrometers, and piezometers were stowed away. In all ordinary weathers, and, in fact, as long as the port which lighted and ventilated the laboratory could be kept open, there was no difficulty in working on board ship.

It seemed very desirable to have some means of checking the depths, as indicated by the sounding line, and it was clear that some sort of barometric method must be used. Air had already been tried without success, and so water was finally adopted. The piezometer used consisted of a thermometer, the bulb having a capacity of about 9 c. c., the stem being 13 inches long and 1 mm. in diameter. The thermometer was inverted, after filling with water, in a little flask filled with mercury. The neck of the flask is connected to the stem by a short piece of India-rubber tubing, between which and the stem a piece of glass rod is inserted, so as to admit the external pressure to the surface of the mercury. By warming the bulb enough water is forced out to allow the mercury, on cooling the apparatus, to rise to a convenient height in the stem. In the stem is a little index similar to those used in Six's thermometer. The stem is divided into millimeters, and was carefully calibrated. If such an instrument be sunk with a sounding line, it is clear that the water will be compressed, and that the mercury with the index will rise, such rise being the sum of the decrease in temperature × the increase of pressure. If a thermometer, properly protected, be sunk at the same time, the temperature will be registered, and thus the increase of pressure can be found. Before starting it was thought that a gauge similar to those used with hydraulic pumps might be employed to determine the pressure, but, on examination, the gauge furnished was found unreliable, and, after all, the length of the sounding line furnished the best gauge of the pressure, provided that care was taken that no currents, bad weather, etc., interfered with the result. If there is a current, its existence is always rendered evident by the behavior of the sounding line as it goes up and down, so that, practically, there is no difficulty in saying when a sounding is good. There was some difficulty with the springs of the indices. As a mean, the compressibility of distilled water per 100 fathoms was found to be 0·0009, or per atmosphere 0·000049, Regnault finding 0·0000486. The results at various depths agreed well: thus, at 1,000 fathoms, 0·00098; at 2,000, 0·00007; at 3,000, 0·0008. The compressibility of sea-water was determined in precisely the same instrument, and was found to be, at 800 fathoms, 0·00077; at 1,000, 0·00078; at 1,500, 0·00073, per 100 fathoms. These experiments were all made by attaching the piezometer to the sounding line, the temperature being from 1° to 3° C. In order to determine the compressibility at higher temperatures, recourse was had to a hydraulic instrument; but as no reliable gauge was at hand, the only results possible were comparative between sea-water and distilled water. At 26·5° C., distilled water = 100, 93·6; 25·8°, 92·0; 22·1°, 92·8; 22·5°, 92·0; 13·7°, 92.6. A solution containing 4 per cent. sodium chloride gave results (distilled water = 100), 26·5°, 90·51; 25·8°, 88·13. 22·1°,

* A paper read before the Chemical Society, February 21st, 1878.

86·61; 13·7°, 86·84. The compressibility of mercury was next determined, and found to be, per 100 fathoms, 0·000271, and per atmosphere, 0·0000015. The apparent compressibilities of water and mercury having been thus determined, the next task was the practical one of applying the results to control the soundings as obtained by the line, and the temperatures as indicated by the thermometer. Two piezometers, one charged with water, the other with mercury, were sent down on the sounding line; now water is very sensitive to changes of pressure, but not to changes of temperature; mercury, on the other hand, is less affected by changes of pressure, but considerably more by variations in temperature. The true temperature and true depth were obtained from the readings of the two instruments, thus: The rough depth as given by the sounding line is applied to correct the temperature as indicated by the mercury piezometer; by this temperature the reading of the water instrument is corrected; thus the correct depth is obtained to within twenty-five fathoms. This is again applied to the mercurial instrument, and so, by correcting backward and forward, both the correct depth and the correct temperature are found. For great depths the piezometer above described is useless, a pressure above 3,000 fathoms forcing the mercury into the bulb. To obviate this difficulty a bulb was blown in the middle of the stem, and its size was so arranged that the contraction up to 1,500 fathoms was covered by this bulb, and so the stem was left to indicate greater pressures. The amounts of oxygen and nitrogen in sea-water were estimated; about 900—1,000 c. c. were used. The sample was collected in a brass cylinder furnished with stop-cocks having a ¾-inch bore, and was transferred to the flask in which it was to be boiled with as little disturbance as possible. To the flask is fitted an India-rubber cork, through which a glass tube, having a large bulb attached to it, passes; the tube and bulb are filled with distilled water, the end of the tube in the flask being closed by a simple device. The bulb is now boiled for ten minutes, and the upper end of the tube closed. The lower end is now connected with the water in the flask, and the latter boiled for one and a half to two hours. The gases evolved are collected in the bulb and analyzed. The carbonic acid was determined by distilling about ¼ liter of water to dryness, after the addition of 15 c. c. of barium chloride solution to prevent bumping. The steam was passed through a still, and the distillate with evolved gases collected in flasks and U-tubes containing standard baryta water. This, by titration with hydrochloric acid, rosolic acid being used as an indicator, gave the amount of carbonic acid. The following are the results of the analyses of about one-third of the gas samples obtained: Oxygen + nitrogen = 100. In surface water O = 33·67; at 25 fathoms, 33·4; 50 fathoms, 32·3; 100 fathoms, 30·2; 200 fathoms, 23·4; 300 fathoms, 11·4; 400 fathoms, 15·5; 800 fathoms, 22·6; below 800 fathoms, 23·5. So that from the surface downward the oxygen decreases to 300 fathoms; below this depth it increases in amount; this increase is in all probability due to the decreasing amount of animal life at depths greater than 300 fathoms. Experiments with the tow net at various depths confirmed this view. As regards superficial distribution, the cold water at the Antarctic regions, both at the surface and below, is thoroughly aerated, the gases diminishing in quantity as we approach the equator, and increasing again as we proceed northward.

From many experiments it was found that the absorption of nitrogen by sea-water varies considerably from that by distilled water, especially at high temperatures. As regards density and depth of sea-water, two regions of maximum density exist north and south of the equator—the north maximum being 25° north of the equator, the south about 15° south. Toward the equator the density at the surface falls off, while toward the Arctic and Antarctic regions the density decreases rapidly.

These regions of maxima correspond to the regions frequented by the trade winds. It is known that the south trade blows much nearer the equator than the north, and so the maximum of density is nearer the equator on the south. As to vertical distribution, the maximum penetrates some depth, but at 350 fathoms a great tract of water of low density is found. At the equator we find first an increase as we go down, then a decrease, and, last of all, again an increase of density near the bottom. The densest water is found in the Atlantic. In the Pacific a certain imitation of the areas of dense water just described is found. Near ice very light water is found; again, near Hong Kong, just after the monsoons, the superficial water is very light, but during the dry season the water becomes more dense. The maxima of density lie in the North Hemisphere to the southwest, in the South Hemisphere to the northwest of the maxima of barometric pressure. It is at these latter spots that the cold dry winds descend from great altitudes and flow outward, taking up moisture from the sea in their course, and not becoming saturated until they are near the equator.

PARAFFINE.

THE raw material of this branch of mineral industry, which is of comparatively recent origin, is a variety of earthy lignite occurring within a small portion of the Saxon Thuringian brown-coal formation between Weissenfels and Zeitz. This contains a fusible hydrocarbon, the so-called Pyropissite of Kengott, which, in the purest condition, yields by distillation as much as 66 per cent. of tar containing paraffine. The pure mineral is, however, only found in nests and patches in the lignite, the average of the coal worked giving 10 per cent. as a maximum of condensable products, but often less. Freshly-raised coal containing a large proportion of water is found to yield more than that which has been dried by exposure to the air. Thus a series of comparative experiments showed that—

Coal containing 68 per cent. of water gave 40 lbs. of tar and 16 lbs. of gas per tub (of about 3 cwts.).
Coal containing 40 per cent. of water gave 33 lbs. of tar and 44 lbs. of gas per tub.
Compressed coal containing 26 per cent. of water gave 24 lbs. of tar and 36 lbs. of gas per tub.

The increased quantity of permanent gases in the drier coals is obtained at the expense of the tar, a result which is to be attributed partly to the higher temperature prevailing during the distillation and partly to the absence of the direct action of hydrogen on carbon. This is owing to the want of water, which, as is well known, is decomposed at a low heat by carbonaceous substances producing hydrocarbons and carbonic oxide.

The retorts used are of two kinds, horizontal and vertical. The former, which are confined to the older works, are of cast iron, 8¼ feet long, and of an elliptical section, 14 inches high and 28 inches broad. These are either set in pairs, each pair being fired separately, or in batteries of from twelve to twenty, laid across the direction of the grate flame. The former plan is generally preferred, as it allows the retorts to be more uniformly heated. The fire-grate extends along the bottom of one of the retorts, the flames pass by a side flue under the second and return over the tops of both. The fronts of the retorts are connected by goose necks of oval section, with a collecting main running along the front of the furnaces which conduct the volatile products to the condensers. These are made of vertical pipes like those ordinarily used in gasworks. A stop-valve is placed on the connection of each retort with the main to prevent the admission of air to the latter during the time of charging. The average amount of coal carbonized in the twenty-four hours is from 6 to 7 cwts., three charges being worked during that time. The coal is spread over the surface in a layer about 4 inches thick; the residual coke when sufficiently good is used in part for heating the retorts, but otherwise it is thrown away or employed for road-making. The coal required for heating the retorts is from 80 to 100 per cent. by volume of that distilled.

The yield of tar varies greatly with the temperature and with the manipulation, the proportion of condensing surface being also of considerable importance. From 250 to 300 square feet is required for each retort, but a notable improvement in yield has been obtained by increasing it to 400 or 500 square feet.

The vertical form of retort which is adopted in all the newer works is a cast-iron tube 16 feet long, from 4 to 6 feet in diameter at the top, and somewhat less at the bottom, to which is adapted a conical discharging piece and a cylinder of about 2 cubic feet capacity, closed by two slide valves for drawing the coke. The center of the retort contains a system of thirty conical rings overlapping each other in the manner of Venetian blinds, built up into a column round a central rod; the apertures between the rings form the passages for the volatile products given off by the coal which is contained in the annular space between the rings and the cast-iron shell. Two large mouthpieces lead the gases from the central column to the condenser. The heating is effected by an external grate fire, the retort being closely surrounded by an external casing of fire-bricks. The work goes on continuously, the charge of 82 bushels of coal requiring about thirty-six hours for complete distillation. The coke is drawn at intervals of an hour to an hour and a quarter, each portion as it leaves the retort being allowed to cool between the slides of the coke-box before being discharged into the air. Latterly cylindrical retorts with fire-brick instead of cast-iron shells have been tried, with fair results. The vertical form of retort, being continuous in action, does five times the work of a horizontal one; at the same time the process is more regularly conducted, and the yield of tar is larger. The average of five years' working showed the produce to be 70 per cent. of the theoretical yield of the coal with vertical, as against 66 per cent. with horizontal retorts.

The best results are obtained when the temperature is such that the gases leave the retorts at 302° or 392° Fahr.; above this point there is a greatly increased production of permanent gases, while at a lower heat the products are principally water and coke.

Another method which has only been tried experimentally is that of distilling in an atmosphere of superheated steam. This was found to give a higher yield both in regard to quantity and quality of the tar, but it has not been adopted in practice.

The product of the first operation is a dark-colored (yellowish brown to black) mass of the consistency of butter, varying in specific gravity from 0·82 to 0·935, fusible at 59° to 86° Fahr. to a brown fluid with dark green fluorescence. The best product is that of a medium density from 0·84 to 0.87, the average yield by rectification being 35 per cent. of benzine and various illuminating oils, 15 per cent. of gas or lubricating oils, 16 per cent. of paraffine, and 10 per cent. of creosote and pitch.

The rectification is conducted according to three different methods:

1. Distillation of the tar; purification of the resulting crude paraffine by caustic soda and sulphuric acid, or by the latter alone, and redistillation of the purified product.

2. Treatment of the tar with sulphuric acid, washing with water, and rectification with slaked lime.

3. Preliminary distillation with ¼ per cent. of lime; treatment of the paraffine distillate with sulphuric acid.

The first of these is the oldest, and in many respects the safest, being adopted for all classes of material, but is attended with considerable loss of paraffine from decomposition during the distillation; the second is only adopted for the treatment of tar of low density; and the third is, in the author's opinion, the most advantageous, giving 16 per cent. of paraffine as against 12 or 15 per cent. by the other methods.

The operation is conducted in large stills containing from 35 to 50 cwts. of tar, the product being divided into two parts known as crude oil and crude paraffine mass, the latter distinction being made when the distillate solidifies when received upon a cold surface. The proportion is usually 40 per cent. of the former to 54 per cent. of the latter. The distillation is pushed to dryness, a residue of porous coke remaining in the still. Twelve hours are required to work off a charge of 40 cwts., after which the still is allowed to cool during the night so as to be ready for filling early in the morning.

Treatment of the Crude Oil.—This is first cleared from cresote by mixing with caustic soda lye and settling; then treated with sulphuric acid to remove resin, ammonia, and other organic bases; next washed with water until completely freed from acid, and finally rectified by fractional distillation into various products, which are known by the following names:

Benzine, specific gravity 0·790, boiling point 194° Fahr.

Photogene, specific gravity 0.800 to 0·805, boiling point 284° to 302° Fahr.

The oils obtained from the crude paraffine are solar oils, with a specific gravity between 0·825 and 0.845, and boiling point between 347° and 392° Fahr. They are of a yellow color, and form an illuminating material of very high class at an exceedingly low price.

Paraffine oils are those above 0·86 specific gravity, and boiling points between 500° and 518° Fahr. They are usually yellow and somewhat viscid, and are used for lubricating machinery. Notwithstanding the name, it is essential that these oils should be free from paraffine; and their lubricating value is improved by subjecting them to extreme cold in winter in order to freeze out the last traces of that substance.

The crude paraffine, after separation of the associated oils, is crystallized in blocks of 40 lbs. weight, the crystals being separated from the liquid oils by hydraulic presses, centrifugal or other machines. The usual method is to effect a first separation in a chambered filter press, which gives a mass of crystalline scales that is subsequently squeezed in a hydraulic press to cakes under a pressure of 200 atmospheres. The brown coloring matter is removed by melting the crude press cake with 10 to 15 per cent. of benzine, and subjecting it to a second pressing. This is repeated with a further addition of benzine, and after finally washing with water and a little sulphuric acid the paraffine is set aside to crystallize. When so purified its melting point is 116° to 118° Fahr.

The composition of brown-coal paraffine differs from that made from the older coals and shales. The latter is nearly of the theoretical percentage composition, carbon 86, hydrogen 14, while the former contains oxygen, the following analysis being that of one of the best samples made in the district of Weissenfels: Carbon 82·41, hydrogen 14·16, oxygen 3·43.

There are at the present time fourteen mineral oil and paraffine factories at work in the district, consuming about 36,000 tons of tar, 5,500 tons of caustic soda, and 2,900 tons of sulphuric acid, the products being—

12,500 tons benzine, photogene, petroleum, solar and clear paraffine oils.
6,000 tons gas and lubricating oils.
5,000 tons hard and soft paraffine.
3,600 tons creosote, pitch, asphalt, and other accessory products.
27,100 tons, equal to 75 per cent. of the weight of tar treated.

The remaining 25 per cent. representing the loss due chiefly to the production of gas and carbonization by repeated distillations. About 3,500 tons of paraffine are converted into candles in the different manufactories.—*L. Grostowsky.*

THE APPLICATION OF AMMONIUM SULPHOCYANIDE IN VOLUMETRIC ANALYSIS.

By J. VOLHARD.

DETERMINATION OF SILVER.

IN a nitric solution of silver to which a soluble ferric salt has been added a permanent redness does not appear on the gradual addition of a dilute solution of ammonium or potassium sulphocyanide until all the silver has been thrown down as a sulphocyanide. If we know how much of the sulphocyanide solution is required to precipitate a known weight of silver we can determine volumetrically the quantity of silver present in any acid argentic solution, the ferric salt serving as an indicator. For the preparation of the standard solution the author uses ammonium sulphocyanide, though Lindemann prefers the potassium salt; both, in dilute solution, are permanent. But the ammonium salt is less frequently contaminated with chlorides, which interfere greatly if present in more than mere traces. The solution may be conveniently made of such a strength that 1 c.c. indicates 1 centigrm. of silver. The weighed quantity of the salt is dissolved in water, and diluted in a graduated flask so far that 7·5 grms. (or 8 grms. if the salt appears very damp) may be contained in each liter. For the precipitation of 1 grm. of silver 0·704 grm. of pure ammonium sulphocyanide is requisite; 0·5 grm. of chemically pure silver is then weighed out, dissolved in 8 to 10 c.c. of pure nitric acid of sp. gr. 1·20, and, after the complete solution of the metal, the nitrous acid is expelled by boiling or prolonged heating on the sand-bath, and the solution is allowed to cool. It is then diluted with 200 c.c. of water, and 5 c.c. of a cold saturated solution of ammonia-iron alum are added. If the color of the ferric salt is perceptible, a little pure colorless nitric acid is added till it disappears. The sulphocyanide solution is then added from a burette. At first a white precipitate is produced, which remains suspended in the liquid like silver chloride, rendering it milky. On the further addition of sulphocyanide each drop produces a blood-red cloud, which quickly disappears on agitation. As the point of saturation is reached the silver sulphocyanide collects in flocks, and the liquid grows clearer without becoming perfectly limpid as long as a trace of silver remains in solution. As soon as all the silver is precipitated the flocculent precipitate quickly deposits and the supernatant liquid becomes quite clear. The sulphocyanide solution is added by drops till this point is attained, and till a very faint light brown color appears in the liquid, which does not vanish on repeated agitation. The color is most easily perceived if the liquid is held, not up to the light, but against a white wall turned away from the window.

For a repetition of the experiment it is convenient to use a silver solution of known strength. For this purpose 10 grms. of pure silver are dissolved in nitric acid in a liter flask, the nitrous acid is expelled, the liquid is allowed to cool, and diluted up to the volume. For use 50 c.c. are taken with a pipette. If the above-mentioned proportions are preserved for 0·5 grm. silver or 50 c.c. of the silver solution, somewhat less than 50 c.c. of the sulphocyanide solution will be required, whence the needful dilution can be calculated. If 48·5 c.c. sulphocyanide have been used, then to every 48·5 c.c. of the solution 15 c.c. of water must be added.

The solution is perfectly permanent, even on being kept for two years. The silver solution must have a decidedly acid reaction from free nitric acid, which it is quite unnecessary to neutralize. Nor is it important that the proportion of acid in different experiments should be approximately equal. It is, however, essential for the constancy of the results that the ferric salt should be in large excess, and should be used approximately in one and the same proportion to the total volume of the fluid. It must also be remembered that the color of ferric sulphocyanide is destroyed by nitrous acid at common temperatures, and by nitric acid on the application of heat. Hence follows the necessity of completely expelling all nitrous acid, and of allowing the liquid to become cold before the operation is begun. The nitric acid used in this process should be keep in the dark.

TITRATION OF SILVER IN PRESENCE OF OTHER METALS.

Copper.—The proportion of copper in an alloy may reach 70 per cent. without in the least affecting the accuracy of the process. Beyond this limit the recognition of the final reaction is somewhat doubtful, since after the precipitation of the silver the liquid is rendered opaque and discolored by the formation of black copper sulphocyanide. The only remedy in such cases is to add a known weight of pure silver to the alloy so as to reduce the proportion of copper below 70 per cent., as is done in Gay-Lussac's process.

Mercury.—In presence of this metal silver cannot be titrated with sulphocyanide solution. This defect is of little consequence, as mercury is readily expelled.

Palladium renders the titration of silver with a sulphocyanide inaccurate, this metal appearing in the result as silver. This is an important circumstance from a technical point of view, since palladium, though frequently present in silver, occurs in very small quantities.

As regards other metals soluble in nitric acid and found along with silver in alloys and ores, lead, cadmium, thallium, bismuth, zinc, iron, and manganese are without influence. The recognition of the final reaction in solutions strongly colored by salts of cobalt or nickel requires some practice. At first a few drops of sulphocyanide will always be added in excess. It is then recommended to titrate back with a solution of silver, when the pure color of the cobalt or nickel will appear so suddenly and distinctly that it will conversely be easy to seize the exact point when the color of the solution is modified to a yellowish brown by the addition of the color of ferric sulphocyanide. When this change of shade has been observed four or five times by titrating backward and forward with exactly corresponding solutions of silver and of sulphocyanide, such a certainty in the recognition of the final reaction will be attained that there can be no doubt as to the addition of a half drop, more or less.

The presence of tin, antimony, and arsenic does not interfere with the accuracy of the process.

A comparison of the results of this method as compared with those obtained by cupellation and by Gay-Lussac's method shows that it is not inferior in accuracy. In comparison with Gay-Lussac's process it has the advantage that the preliminary cupellation is dispensed with, and the end of the reaction is more distinctly recognized.

DETERMINATION OF THE HALOGENS.

For these determinations two standard solutions are required, one of silver and one of sulphocyanide. For the former 10·8 grms. of chemically pure silver are dissolved in pure nitric acid (sp. gr. 1·20) in a liter flask, and after the expulsion of nitrous acid made up to 1,000 c.c. A solution of ammonium sulphocyanide prepared as above directed is adapted to this silver solution, so that when equal volumes of both are mixed in presence of a ferric salt there may appear a coloration just perceptible. Both solutions are, of course, decinormal.

The determination of chlorine is conducted as follows: The weighed sample of chloride is dissolved in 200 or 300 c.c. of water, mixed with 5 c.c. of the ferric solution, and so much nitric acid as may cause the color of the ferric salt to disappear. A measured quantity of silver solution, rather more than necessary to precipitate all the chlorine, is then added, and without filtering off the silver chloride the sulphocyanide is then dropped in with the burette, taking care to insure the immediate mixture of the drops with the fluid by constant agitation. As soon as the solution takes a pale yellowish brown color, which, after agitation, remains permanent for ten minutes, all the silver is precipitated, and the liquid contains no perceptible trace of chlorine in solution. The number of the c.c. of silver solution added, less the number of c.c. of sulphocyanide solution consumed, multiplied by 0·00355, gives the weight of the chlorine in grammes.

If the amount of the chlorides is totally unknown, so that we do not know approximately how much silver solution is to be added, the sample is mixed with ferric solution and nitric acid as before, and while the silver solution is gradually run in from the burette a drop of sulphocyanide solution is added from time to time. As long as there is still chlorine in solution the redness produced disappears but slowly and gradually, but as soon as the color produced by a new drop of sulphocyanide disappears immediately on agitation, and the liquid appears of a pure milk-white, enough silver has been added, and the titration is completed as directed above. Of course the quantity of sulphocyanide added at first or during the introduction of the silver must also be deducted from the silver solution. It is therefore well to drop in the sulphocyanide with the same burette which is used for the completion of the titration.

Bromine.—The dilute solution of the bromide is mixed with 5 c.c. of iron solution and a little nitric acid, and strongly colored with a few drops of sulphocyanide solution. The silver solution is then added from a burette till complete decoloration is produced, and the titration is completed with sulphocyanide, which is added till the liquid takes a faint but permanent pale brown coloration. The number of c.c. of silver solution added, less the number of c.c. of the sulphocyanide solution consumed, multiplied by 0·0080, gives the weight of the bromine present in grammes.

Iodine.—The iodine is dissolved in 200 or 300 parts of water, preferably in a glass with a well-fitting stopper, and the decinormal solution of silver is added from a burette. As long as iodine remains in solution the silver iodide remains in a state of milky suspension, and even persistent agitation does not clear the solution. When, by the gradual addition of the silver solution, all the iodine has been thrown down, the precipitate becomes collected in flocks, and as soon as a minimum excess of silver is present the liquid appears to coagulate, and on shaking the flocculent silver iodide separates from the limpid liquid. By this phenomenon the conclusion of the reaction is indicated. From one-tenth to two-tenths of a c.c. of the silver solution must then be superadded, the flask stoppered, and shaken for some minutes. In this manner any soluble iodide which has been entangled in the precipitate is converted into silver iodide. Iron solution to the extent of 5 c.c. is then added along with a little nitric acid, and sulphocyanide solution is added with the burette till the liquid is very distinctly colored. On shaking this color disappears. Small quantities of sulphocyanide are then added with alternate agitation till a slight but permanent color is obtained. The number of c.c. of silver solution consumed, less that of c.c. of sulphocyanide used, and multiplied by 0.0127, gives the weight of the iodine in grammes.

DETERMINATION OF THE HALOGENS IN ORGANIC COMPOUNDS.

When the organic matter is previously destroyed by ignition with lime, or, preferably, with carbonate of soda and saltpeter, the application of the volumetric process is too simple to require a special description.

RECOGNITION AND DETERMINATION OF THE HALOGENS IN PRESENCE OF SULPHOCYANIDES.

Silver sulphocyanide is destroyed by heating with concentrated sulphuric acid, and is readily dissolved by the addition of a little nitric acid. If the sulphuric acid is diluted with half its bulk of water it does not attack silver chloride, while its action upon silver sulphocyanide is scarcely lessened. If it is therefore required simply to detect or determine chlorine in presence of sulphocyanides, the solution of the latter, acidified with nitric acid, is precipitated with excess of silver solution. The precipitate, filtered and drained, is placed along with the filter in a boiling flask and covered with pure sulphuric acid of the strength above mentioned. If, on heating, it becomes black and clotty, a few drops of nitric acid are carefully added. The silver chloride, if present in quantity, remains substantially undissolved; if but little chlorine was present complete solution may ensue, but on dilution with water silver chloride is deposited, free from silver sulphocyanide.

In order to detect and determine bromine or iodine in presence of sulphocyanides, the latter are destroyed by fusion with saltpeter and sodium carbonate. It must be remembered that the reaction is extremely violent, and that unless a large proportion of the carbonate is used deflagration may ensue. It is preferable to introduce the mixture gradually into the ignited crucible. The sulphocyanide is mixed with 10 to 15 parts of dry sodium carbonate, and the mixture, having been dried by gently heating in a porcelain crucible and pulverized afresh, is gradually added to 8 to 10 parts of melted saltpeter.

SEPARATION OF SILVER CHLORIDE AND IODIDE.

Silver chloride, when treated with ammonium sulphocyanide in an ammoniacal liquid, is readily and completely converted into silver sulphocyanide, while silver iodide remains untouched.

Cyanogen.—Silver cyanide is converted into sulphocyanide by hydrosulphocyanic acid so rapidly that dissolved silver cannot be titrated with sulphocyanide solution in presence of silver cyanide. But if the silver cyanide be precipitated from a given quantity of prussic acid by an excess of silver solution, there is no difficulty in titrating the excess of silver in the filter with sulphocyanide solution. For the determination of hydrocyanic acid, however, the author prefers Liebig's original process, using a centinormal silver solution. He points out that dilute solutions of hydrocyanic acid are much more volatile than had been supposed.

DETERMINATION OF COPPER.

Copper is precipitated as a sulphocyanide of constant composition in presence of powerful reducing agents, such as sulphurous acid. From the solution of a copper salt, not too acid and mixed with sulphurous acid, the copper is completely precipitated by ammonium sulphocyanide. A gentle heat promotes the formation and separation of the precipitate. The quantity of the copper is found by precipitating with a known excess of sulphocyanide solution, and then determining with silver solution, after addition of ferric salt and nitric acid, the quantity of sulphocyanide remaining in solution.

The standard solutions of silver and sulphocyanide are decinormal as used for the determination of the halogens. The sulphurous acid does not interfere with the distinctness of the final reaction if ferric salt has been added in sufficient quantity.

For the titration of mercurial compounds the process is less exact and is not recommended by the author.

The oxidizing power and value of dilute permanganate solutions can also be determined by means of a solution of sulphocyanide titrated with silver.—*Liebig's Annalen der Chemie.*

OXYGEN MAKING.

THERE is much truth in the adage, "Familiarity breeds contempt;" and it is well that it should be so, as, were it not for that, there are many occupations of such a dangerous nature that it would be difficult to find men willing to undertake them. But while this becoming accustomed to danger is so far good, it is quite possible to overdo it and neglect simple precautions, thereby converting what would otherwise be safe operations into work which is too frequently followed by disaster and death. This fact is abundantly proved by the frequent explosions permitted to occur in the production of oxygen from potassium chlorate and manganese dioxide. It is well known that, so long as the material is pure and the tubes clear, the operation may be carried on with perfect safety; but when the manganese has been, either by accident or design, mixed with carbon in one or other of its various forms, it results in the formation of carbonic acid in such volume as to make an explosion inevitable. How carbon finds its way into manganese dioxide is difficult to understand, the price of the latter being such as to put intentional adulteration out of the question. The fact is, nevertheless, patent that it is not unfrequently present; and, as a simple and ready means of discovering it has been again and again pressed on the attention of those interested, it seems little short of a crime to neglect it.

There is no better or readier method of ascertaining the safety or otherwise of the mixture about to be used than the one I published in this journal more than a dozen years ago, which was this:—On getting a fresh stock of manganese mix a few grains of it with the suitable proportion of the chlorate, place the mixture in a test tube, and hold it over a gas or lamp flame. If the material be sufficiently pure the salt will be decomposed and the gas pass quietly off. There may be seen a few bright sparks of light playing on the surface of the mixture, arising probably from the presence of minute traces of carbonaceous matter, but they are of no consequence. Should, however, there be carbon present in sufficient quantity to cause an explosion, as soon as decomposition has fairly commenced the contents of the tube will be projected in the form of flame and incandescent particles like the burning of a squib, but much more violently. I need hardly say that the mouth of the tube must be turned away from the operator, and if that be attended to there is not the slightest danger in the application of the test.

While carbon is the general source of danger in the production of oxygen, much more dangerous substances are occasionally met with, and readers of this journal will remember an instance that occurred in Leven, in which sulphide of antimony had been supplied instead of manganese; but the above test is applicable to this or indeed to any other substance from which an explosion can arise.

In a recent article on this subject the editors directed attention to a hitherto little-suspected source of explosion, viz., the alleged fact that a drop of the oil that had been applied to the screw of the mercury-bottle form of retort might fall into the heated mixture and lead to the liberation of olefiant gas. No doubt if such were the case a serious accident might occur, as olefiant gas mixed with three times its volume of oxygen explodes with great violence; but the editors will not be very angry if I say to those who still use that obsolete and unsuitable form of retort that they may keep their minds at rest, as the temperature required for the decomposition of the salt is far too low to break up the oil, and every trace of oxygen would be off long before an atom of olefiant gas could be produced. I am aware that the late Alexander Bryson, of Edinburgh—no mean authority on such matters—at a meeting of the Royal Scottish Society of Arts, attributed an explosion that occurred in his house to that cause; but he soon afterward saw reason to change his opinion, as the tube, which is still in my possession, was found completely choked. It had been filled with metal to enable a suitable bend to be made, and that metal remains there to this day.

Of course all this has been stated before, but some things require to be repeated again and again before those especially interested can be induced to give them their attention, as a case that occurred here a few days ago clearly shows. A well-known member of the Edinburgh Photographic Society—who has for many years been in the habit of making oxygen for lantern purposes, and who, according to his own account, had never had an explosion, and did not think he was likely to have one—sent for the usual supply of material and placed about a pound and a half in a new conical sheet-iron retort. This was placed on an open fire in a grate or stove, and in a few minutes an explosion occurred, tearing the retort as if it had been cardboard, wrenching the exit tube completely off at the junction with the cover, breaking the grate into fragments, throwing himself violently against the opposite wall of the room, and also smashing the glass in the window. Now all this would have been prevented by the application of the above simple test, as on examining the manganese it was found to contain over twenty per cent. of coal dust.

In the ordinary manufacture of oxygen, especially when the convenient Bunsen burner is the source of heat, I have reason to believe that many operators do not get the full quantity of gas the salt contains. Experience shows that, practically, a pound of the chlorate will yield about 4·8 feet, but I know that too often much less is obtained. Shortly after the flame is applied the oxygen begins to come off—slowly at first, but in a minute or so very rapidly—and continues to do so for some minutes. Gradually it decreases in rapidity, and by and by comes only in single bubbles. If the heat be continued, and especially if a little increased, the gas will in a few minutes again come off as rapidly as at first, continue for a longer time to do so, and return to single bubbles, or perhaps stop altogether. Now if at this stage the heat be withdrawn, the tubes disconnected, and the retort allowed to cool and then opened and inverted, a considerable quantity of undecomposed crystals will fall out mixed with a large proportion of manganese; and, as the usual method is to pour water into the heated retort to dissolve the chloride residue, this undecomposed chlorate is, of course, lost. The heat, therefore, should be continued, and it will be found that, in most cases at least, a third liberation of oxygen will take place. No doubt the difficulty of getting off the full quantity of oxygen arises to a great extent from too large a quantity of the mixture having been put into the retort, the heat which plays only on the bottom being insufficient to decompose the upper layer. The ordinary form of retort is also faulty; it would be much improved by being at least one-half wider at the base.

My attention was particularly directed to this subject a short time ago, on being consulted by the manager of a large theater, who employed from eight to twenty lime lights in certain scenes in a pantomime. The oxygen was passed direct from the retort into a large iron tank, from which, under suitable water pressure, it was supplied to the burners; but the consumption was so great that the retort had to be kept constantly at work during most of the evening to keep up the supply. The retort, which was of thin boiler plate, was suspended from the roof of the tank house by a chain immediately over a rose Bunsen burner, the flame of which was large enough to cover the bottom and come well up the sides. The stock of "oxygen mixture" was kept in packets of five pounds each—four of chlorate and one of manganese—and the charge was one such packet. This should have given at least over 17 feet of gas, but as I saw the operation carried on the product was not much more than half that quantity. This great loss was not discovered, as to save time the moment the heat was removed water was poured in to dissolve the supposed residue, and enable the gas-man to start another batch. I recommended the reduction of the charge to one-half, and I understand that they now get as much oxygen at each operation as before, and that it comes off in less than half the time previously required.

Acting on a hint given by the editors in the article already alluded to, I have gone pretty fully into the question of the advantages likely to be derived from the substitution of some inert material for the manganese, and am forced to the conclusion that the action of the latter is more than simply mechanical. The experiments were made in a hard glass tube, one gramme of potassium chlorate being employed in each, and the heat—that of a small Bunsen burner—was the same in all cases. The chlorate, without admixture of any kind, commenced to fuse at the end of one minute, and shortly after bubbles of gas came off, the fused mass spurting a good deal half way up the tube. At the end of four minutes seventy cubic centimeters of oxygen had been collected, and it continued to come off slowly; but the tube showed indications of being about to fuse, and the flame was withdrawn. When the tube had cooled the residuum was very hard and dissolved with difficulty, so that the process of emptying a retort would be troublesome.

In the next experiment the chlorate was intimately mixed with a nearly pure silica in the shape of the beautiful white Fontainebleau sand used in the manufacture of the finer varieties of flint glass. Within thirty seconds after the application of heat the mass acquired a pasty consistence, and rapidly swelled till the tube was half full. When the gas began to come off there appeared a number of beautiful scintillations, showing the presence of carbonaceous matter in small quantity. It stopped when 160 c.c. had been collected, and no more could be got until the lapse of four minutes, at which time the glass had passed into a bright red and was beginning to bulge.

In the third experiment ordinary river sand was used, and although the result was better, it was not satisfactory. The objectionable swelling up did not take place, but the salt seemed to separate from the sand, collecting at the bottom of the tube, and behaving not unlike the fused chlorate in the first experiment, and ceasing to give up gas when 240 c.c. had come over.

In the last experiment the usual manganese was mixed with the chlorate, and the result was in every way satisfactory. The oxygen began to come off about twenty seconds after the heat had been applied, and continued steadily to do so for two minutes, at which time the receiver showed 300 c.c. The mass did not pass beyond a pasty state; it did not appreciably increase in bulk, and the salt may be presumed to have been completely decomposed, as it stopped at that stage and did not again appear, although the heat was applied till the tube actually fused.—JOHN NICOL, Ph. D., in *British Journal of Photography.*

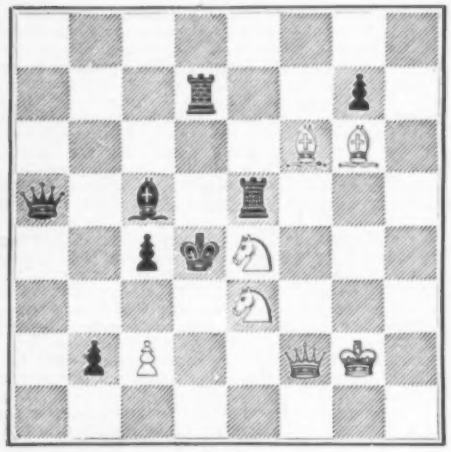
SCIENTIFIC AMERICAN CHESS RECORD.

[All contributions intended for this department may be addressed to SAMUEL LOYD, Elizabeth, N. J.]

PROBLEM No. 70. By CONRAD BAYER.

First Prize in British Association Tourney, 1862.

Black.



White.

White to play and mate in three moves.

CHAS. H. WATERBURY.

White to play and mate in 2 moves.
By LOUIS QUIEN.

BY the introduction of Mr. Waterbury to our readers we make them acquainted with the genial features of one whom by reputation they have long known as one of the best analysts and expert solvers which this or any other country can boast.

He has been one of our special cronies for about a quarter of a century, and although he has not composed many problems during the last score of years, we remember when he belonged to the very foremost ranks, and as a solver was always looked upon as a terror to careless composers.

He takes special delight in solving the most difficult positions, and has succeeded in cutting down some of the colossal problems that have been towering over our problemists for so many years.

He is a most inveterate wag and punster, bubbling over with wit and humor, and always bound to have his little joke on you, yet so careful and conscientious in his views that we have never known him to give a hasty opinion or one that would give offense; as a critic he is sound and scientific, and often as we have sought his counsel on problematical questions, we have always found his views to be correct.

His initials have got a little mixed up with our contributor C. H. Wheeler, but the following, which was published by "C. H. W.," gave us considerable trouble in our younger days, and savors much of the style of the veteran about it.

ENIGMA No. 38.—By C. H. WATERBURY, of Elizabeth, N. J.

White.—K K R 5, Q Q R 4, R K R 3, Kts Q R 5 and Q B 5, Ps Q R 6 and Q B 6.
Black.—K on K 4.
White to play and mate in three moves.

BRITISH ASSOCIATION TOURNAMENT OF 1862.

THIS grand international tournament was held in London during the months of June and July, 1862; fourteen players entered, who had to contest one game exclusive of "draws" with every other player.

The following were the names of the competitors and shows the result of the contest:

Anderssen :	12	games.....	First Prize.....£100
Paulsen:	11	"	Second Prize 50
Owen:	10	"	Third Prize..... 30
McDonnell:	9	"	Fourth Prize... .. 15
Dubois:	9	"	Fifth Prize. 10
Steinitz:	8	"	Sixth Prize 5
Barnes:	7	"	
Hannah:	7	"	
Lowenthal:	4	"	
Blackburne:	4	"	
Deacon:	3	"	
Mongredien:	3	"	
Green:	2	"	
Robey:	2	"	

A handicap tournament was then inaugurated in which odds were given by the stronger players. The winners were McKenzie, First Prize, £60; Deacon, Second Prize, £30; Anderssen, Third Prize, £15; Hannah, Fourth Prize, £15; Solomons, Fifth Prize, £10; Green, Sixth Prize, £5.

A match was played between Anderssen and Paulsen which resulted in a drawn battle by a score of 3 to 3. Paulsen and Blackburne each gave a blindfold exhibition of playing ten games simultaneously.

As a specimen of the play we give the decisive game between Anderssen and Paulsen.

There was also a problem tournament which owing to the number and value of the prizes elicited an enormous competition.

Four hundred and fifty-two problems were received, and the following was the award:

First Prize, for set of six problems. C. Bayer, £20.
Second Prize. Herr J. Plachutta, £10.
Third Prize. Dr. A. Novotny, £5.
Fourth Prize. Rev. G. McArthur, £2 10s.

A special prize, offered by Mr. H. Waite, of £10, for the best single problem—Conrad Bayer. For the best four selfmates, £5—Count Pongracz. For the best six end games, £10—Herr Horwitz.

We select one of the winning problems from the two best sets; the problem which received the prize as being the best problem of the tournament is the same which was subsequently entered by its distinguished author in the Paris Tournament of '68, and which we published as Enigma 4.

An incident worth mentioning in connection with the problem tournament of '62 is that there was a selected committee of examination and award, who at first awarded the prize to J. G. Campbell, the distinguished English composer.

After the award had been made and the money paid to Mr. Campbell, it was discovered that his set was faulty; a second examination and award was made, still giving the prize to Mr. Campbell for his remaining problems, but the public discovered that the second award was made to problems that were incorrect, therefore a more thorough examination was made and the final award determined upon.

A demand was then made upon Mr. Campbell for the return of the money which had been paid to him, but as he had devoted a large portion of it toward a proper celebration of the event, he very justly argued that it was not his fault, and he should not be made to pay for the inefficiency of the committee of examination. The committee very properly therefore made a levy upon their own pockets and paid the amount of the prizes a second time.

CHAS. H. WATERBURY.

BRITISH ASSOCIATION TOURNAMENT OF 1862.

A. ANDERSSEN.	L. PAULSEN.
WHITE.	BLACK.
1. P to K 4	1. P to K 4
2. Kt to K B 3	2. Kt to Q B 3
3. B to Q Kt 5	3. Kt to K B 3
4. P to Q 3	4. P to Q 3
5. B x Kt ch	5. Kt P x B
6. P to K R 3	6. K B to K 2
7. Q Kt to B 3	7. Castles.
8. Castles.	8. Kt to K sq
9. P to Q 4	9. P x P
10. K Kt x P	10. Q B to Kt 3
11. B to K 3	11. P to Q 4
12. K Kt to K B 5	12. K B to B 3
13. B to Q B 5	13. Kt to Q 3
14. K R to K sq	14. K R to K sq
15. Q to K Kt 4	15. Kt x Kt
16. P x Kt	16. Q to Q 2
17. Q to K B 3	17. P to Q R 4
18. Kt to K 2	18. P to Q R 5
19. P to Q B 3	19. R to Q R 4
20. B to Q 4	20. Q to Q 3
21. Kt to Kt 3	21. B to K 4
22. P to K B 6	22. Q x P
23. Q to K R 5	23. P to K Kt 4
24. Q to K 2	24. Q to K R sq
25. B x B	25. P to K B 3
26. Q to K R 5	26. R x B
27. R x R	27. P x R
28. Q to K 8 ch	28. K to Kt 2
29. Kt to B 5 ch	29. Resigns.

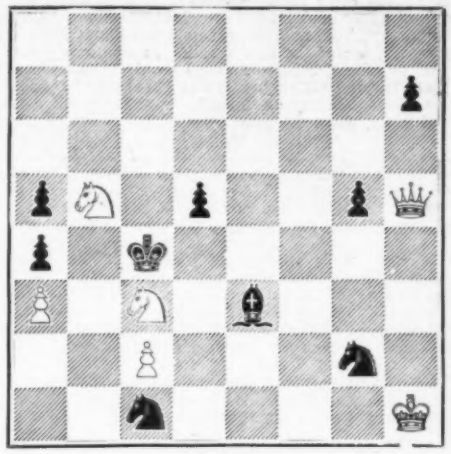
DISTINGUISHED CHESS PLAYERS.

CHESS has included among its votaries the following celebrated men:—Burton (Anatomy of Melancholy), Earl Chatham, Helvetius, Commenius, the grammarian; Condé, the French general; Cowley, Denham, Lydgate, poets; Sir W. Jones, Leibnitz, Charles XII. of Sweden, Sir Walter Scott, Raleigh, Rousseau ("Jean Jacques"), Voltaire, Frederic the Great, S. Warren, Warton, Benjamin Franklin, Buckle, the historian; Tamerlane, Richelieu, Edmund Burke; Wollaston, Chamfort, St. Foix, Marmontel, Grimm, Marshal Saxe, Philidor, the musical composer; Dr. Roget, the mathematician; Ferdinand of Arragon, Holbach, Diderot, D'Alembert, A. Jaubert, Duc de Bassano, Murat, Marshal Berthier, Eugene de Beauharnais. Most of Napoleon's marshals were chess players. In philosophy, science, and art it claimed such representatives as Lord Bacon, Euler, Shumacher, Wolff, and Tomlinson. The eminent astronomer, Mr. R. A. Proctor, is known as a chess problem composer. Robespierre, the French revolutionist, was very partial to the game. Henry I. of England was a chess player. So was each of the following great names: Charlemagne, Sebastian, King of Portugal; Philip II. of Spain, and his favorite prelate, the celebrated Ruy Lopez, the chess bishop; the Emperor Charles V., Catherine de Medicis, Pope Leo X., Henri Quatre, Queen Elizabeth, Louis XIII., Louis XIV., James I. of England, William of Orange, the witty Sydney Smith, General Haxo, Méry, the poet; Lacretéle, the naturalist; Flaxman, the sculptor; Sir W. Jones, poet and linguist; Vida, the Latin poet; the late eccentric Duke of Brunswick, who was a strong player and a liberal patron of the game; the late Howard Staunton, whose knowledge of Shakespeare and Elizabethan literature was as great as of chess; the late Mortimer Collins, poet and novelist; Cunningham, the historian; Kempelen, the mechanician; Staudigl, the opera singer; Hyde, Douce, Barrington, Sir F. Madden, Professor D. Forbes, Orientalist; Admiral Tchichakoff, who opposed Napoleon's passage of the Beresina; Sir John Harrington, the learned and brilliant godson of Queen Elizabeth. The late Lord Lyttelton was a strong chess player, and was president of the British and Counties Chess Associations. Baron Rothschild, of Vienna, one of the wealthiest men in the world, is a lavish supporter of chess; and so was the late American millionaire, Commodore Vanderbilt. M. Grévy, the eminent French statesman and late president of the Versailles Assembly, is a skillful chess player. Coming nearer home, we find that the Hon. Graham Berry, the protectionist Premier of Victoria, is also the premier player of Geelong. Lord Lytton, the Viceroy of India, has a strong liking for the game; and the two Princes Ouroussoff, of Russia, are players of the first grade. The Duke of Wellington was a chess player of more than the moderate skill of his great rival—Napoleon. The late lamented Prince Albert cultivated the princely pastime. Her Majesty the Queen also plays chess, and H. R H. Prince Leopold distinguished himself at Oxford as an excellent player. The late Napoleon III. patronized the game, and gave a trophy, known as the "Emperor's Prize," at the Chess Congress in Paris in 1867. The Emperor Francis Joseph imitated his example at the Chess Congress in Vienna in 1873.—*Adelaide (Australia) Observer.*

PROBLEM No. 71. By SIGNOR JOSEPH PLACHUTTA

Second Prize in British Association Tourney, 1862.

Black.



White.

White to play and mate in four moves.

SOLUTIONS TO PROBLEMS

No. 64.—By SAMUEL LOYD

WHITE.	BLACK.
1. Kt to K Kt 4 ch	1. K to R 8
2. Q to K R 2 ch	2. P x Q
3. Kt to B 2 mate.	
	1. K to R 6
2. Kt to R 2	2. Moves
3. R or Q to K R 8 mate.	
	1. K to B 6
2. Q to Q B 2	2. P to Kt 7
3. Q to Q 3 mate.	
	1. K to B 8
2. R to Q R 8	2. Any move
3. R to Q R sq mate.	

No. 65.—By SAMUEL LOYD.

WHITE.	BLACK.
1. B to Q B 7	1. K x R
2. B to Q Kt 5	2. K to B 4
3. R to K 5 mate.	
	1. K to K 6
2. B to K Kt 3	2. K to K 5
3. R to K 5 mate.	

LETTER "H."—By R. H. SEYMOUR.

WHITE.	BLACK.
1. Kt to K B 6	1. P x R
2. Kt to K B 7	2. P to Kt 4
3. R x P mate.	
	1. K to R sq
2. B to B 8	2. P x R
3. Kt to B 7 mate.	

ENIGMA No. 35.—By C. H. WHEELER.

WHITE.	BLACK.
1. B to K 7	1. K x P
2. K to B 7	2. K to R 3
3. B to Q 8	3. K to R 4
4. K to Kt 7 dis mate.	